UNA FIDES ALTARE COMMUNE

TRANSACTIONS

OF THE

NEW YORK

ACADEMY OF MEDICINE,

INSTITUTED 1847.

SECOND SERIES. VOLUME TWO.

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1876.

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1867. ALFRED C. POST, M. D., LL. D.
1869. HENRY D. BULKLEY, M. D.
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1847.	FRANCIS U. JOHNSTON, M. D.	1856.	JACKSON BOLTON, M. D.
1847.	THOMAS COCK, M. D.	1857.	JAMES R. WOOD, M. D.
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1848.	WILLIAM W. MINER, M. D.	1858.	S. CONANT FOSTER, M. D.
1849.	ISAAC WOOD, M. D.	1859.	JOEL FOSTER, M. D.
1849.	JAMES R. MANLEY, M. D.	1860.	MOSES D. VAN PELT, M. D.
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1850.	ALFRED C. POST, M. D.	1867.	ISAAC E. TAYLOR, M. D.
1851.	JOHN P. BATCHELDER, M. D.	1868.	EDMUND R. PEASLEE, M. D.
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1854.	JOHN H. GRISCOM., M. D.	1875.	GOUVERNEUR M. SMITH, M.D.
1856.	JOHN WATSON, M. D.	1876.	FORDYCE BARKER, M. D.

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1862. S. CONANT FOSTER, M. D.
1863. JOHN W. DRAPER, M. D., LL. D.
1866. JOHN ORDRONAUX, M. D.
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1868. AUSTIN FLINT, M. D.
1869. GOUVERNEUR M. SMITH, M. D.
1872. EDWARD S. DUNSTER, M. D.
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INSTITUTED 1847.

UNA FIDES ALTARE COMMUNE.

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☞ The figures following the words *Trans.*, *Bul.*, and *Pro.* denote the number of papers which have been contributed by the Fellows, and printed in the volumes of the "Transactions," "Bulletin," and "Proceedings," already published by the Academy.

Those marked thus * have deceased.

JULY, 1876.

ELECTED.

Original. ADAMS, JOHN G., M. D., R. S. 1850–'51; Trust. 1852. C. S., 1849, 1855, 1862, and now in office. *Pro.* 1.

1859. AGNEW, CORNELIUS R., M. D., Clinical Professor of Diseases of the Eye and Ear in the College of Physicians and Surgeons, N. Y.; Surgeon to the Manhattan Eye and Ear Hospital, N. Y.; Consulting Surgeon to the Brooklyn Eye and Ear Hospital. *Bul.* 2.

1875. ALLIN, CHARLES M., M. D.; Surgeon to the New York Eye and Ear Infirmary; Surgeon to the New York Hospital.

Original. ANDERSON, JAMES, M. D., V. P. 1852; P. 1861–'67. Trust. 1871–'76. *Bul.* 1; *Pro.* 1.

ELECTED.

1867. ANDERSON, JAMES H., M. D., Surgeon to the Home for the Aged and Indigent Blind, N. Y.

1864. ANDERSON, WILLIAM C., M. D., Surgeon to S. R. Smith Infirmary, Edgewater, Stapleton, Staten Island, N. Y.

1847. ANDREWS, JARVIS M., M. D., late Surgeon of Police.

1862. ARNOLD, E. S. F., M. D., Newport, R. I.

1876. ARNOLD, J. W. S., M. D., Professor of Physiology and Histology in the University of the City of N. Y.

1875. AYRES, SAMUEL, M. D.

1865. BAHAN, THOMAS S., M. D., late Physician to the Northern Dispensary, N. Y.

1867. BALL, A. BRAYTON, M. D., Lecturer on Diseases of the Kidneys in the College of Physicians and Surgeons, N. Y.

1862. BANKS, JAMES L., M. D., Physician to the Presbyterian Hospital; Consulting Physician to the Presbyterian Home for Aged Women, N. Y.; President of Society for the Relief of Widows and Orphans of Medical men, N. Y. Trust. 1867, now in office.

1854. BARKER, B. FORDYCE, M. D., Professor of Clinical Midwifery and Diseases of Women in Bellevue Hospital Medical College; Physician to Bellevue Hospital, N. Y. V. P. 1857. 1876, now in office. *Trans.* 5; *Bul.* 3.

Original. * BARKER, LUKE, M. D., *Obit.* 14th December, 1849, *æt.* 59.

1854. BARRY, ROBERT A., M. D., late President of the East River Medical Association.

1862. BARSTOW, JOSIAH W., M. D., Physician to Sanford Hall Insane Asylum, Flushing, L. I.

Original. * BATCHELDER, JOHN P., M. D., late Professor of Surgery in Berkshire Medical Institution, Pittsfield, Mass.; P. 1858; V. P. 1851–'52. *Obit.* 7th April, 1868, *æt.* 83.

1868. BAYLES, GEORGE, M. D., Physician to Northwestern Dispensary, N. Y.

Original. BEADLE, EDWARD L., M. D., Vice-President of the College of Physicians and Surgeons, N. Y., late Physician to the New York Lying-in Asylum; V. P. 1853–'57; Trust. 1851; C. S. 1850–'52.

Original. BEALES, JOHN C., M. D., Consulting Physician to the Presbyterian Home for Aged Women, N. Y.

Original. * BEALS, GORHAM, M. D., Physician to the New York Dispensary. *Obit.* 9th January, 1848, *æt.* 29.

ELECTED.

1870. BEARD, GEORGE M., M. D., late Physician to the Demilt Dispensary, N. Y. *Bul.* 1.

Original. * BECK, JOHN B., M. D., Professor of Materia Medica and Medical Jurisprudence in the College of Physicians and Surgeons; V. P. 1847 and 1848. *Obit.* 9th April, 1851, *æt.* 56.

Original. * BEDFORD, GUNNING S., M. D., Professor of Obstetrics in the University of the City of New York. *Obit.* 5th September, 1870, *æt.* 64.

1862. BELDEN, EBENEZER B., M. D.

1863. BELL, AGRIPPA N., M. D., Physician to the Brooklyn City Hospital, Brooklyn, L. I. *Bul.* 2.

1871. BELL, CHRISTOPHER B., M. D., Curator of New York Hospital. *Bul.* 1.

1864. * BIBBINS, WILLIAM B., M. D., Physician to the Demilt Dispensary; Trust. 1867–71. *Obit.* 16th January, 1871, *æt.* 46.

1871. BILLINGTON, CORNELIUS E., M. D., Physician to the Demilt Dispensary, N. Y. *Trans.* 1.

1872. BLAKE, JOHN ELLIS, M. D.

Original. BLAKEMAN, WILLIAM N., M. D.

Original. * BLISS, JAMES C., M. D., V. P. 1850. *Obit.* 12th August, 1855, *æt.* 64.

Original. BLIVEN, JEREMIAH P., M. D.

Original. * BLOIS, SAMUEL, M. D., *Obit.* 19th October, 1873.

1871. BLUME, SAMUEL, M. D.

1857. BLUMENTHAL, MARK, M. D., President of the Institution for Improved Instruction of Deaf Mutes, N. Y. *Bul.* 1.

1873. BOGERT, CORNELIUS R., M. D., Medical Examiner of the New York Life Insurance Company.

Original. * BOLTON, JACKSON, M. D., V. P. 1856; R. S. 1852. *Obit.* 16th February, 1866, *æt.* 51.

1848. * BOORAEM, AUGUSTUS C., M. D., *Obit.* 16th December, 1871, *æt.* 46.

Original. * BOYD, THOMAS, M. D., *Obit.* 18th March, 1856, *æt.* 83.

1869. BOZEMAN, NATHAN, M. D.

1876. BRADLEY, EDWARD, M. D., late Surgeon to the Demilt Dispensary, N. Y.

1847. * BRADY, PATRICK J., M. D., *Obit.* 23d October, 1856, *æt.* 42.

1852. * BROOKS, GEORGE W., M. D., *Obit.* 1853.

1863. BROWN, D. TILDEN, M. D., Physician to the Bloomingdale Asylum for the Insane, N. Y.

ELECTED.	
1867.	* BROWN, JAMES L., M. D., Physician to the Demilt Dispensary, N. Y. *Obit.* 4th February, 1873, *æt.* 41. *Trans.* 1.
1848.	BROWN, WILLIAM K., M. D., Brooklyn, L. I.
1848.	BRUENINGHAUSEN, CHARLES, M. D.
1875.	BRYANT, JOSEPH D., M. D., Surgeon to the Northwestern Dispensary, N. Y.
Original.	BUCK, GURDON, M. D., Surgeon to the New York and Presbyterian Hospitals; Consulting Surgeon to St. Luke's and Roosevelt Hospitals, N. Y.; C. S. 1848; V. P. 1852, 1855, 1856, 1859, 1860. *Trans.* 7; *Bul.* 8.
1855.	BUDD, CHARLES A., M. D., Professor Emeritus of Obstetrics and the Diseases of Women and Children in the University of the City of New York; Physician to Bellevue and Mt. Sinai Hospitals, N. Y. *Bul.* 1.
Original.	* BULKLEY, HENRY D., M. D., Physician to the New York Hospital; P. 1870–'71; V. P. 1862–'68; Trust. 1871. *Obit.* 4th January, 1872, *æt.* 67. *Bul.* 2.
1874.	BULKLEY, LUCIUS D., M. D., Physician to the Demilt Dispensary, N. Y.
1848.	* BULLUS, EDWARD S., M. D., *Obit.* 25th September, 1854, *æt.* 50.
1856.	BUMSTEAD, FREEMAN J., M. D., late Professor of Materia Medica in the College of Physicians and Surgeons, N. Y. *Bul.* 1.
1861.	BURKE, JOHN, M. D.
1865.	BURRALL, FREDERICK A., M. D., Physician to the Presbyterian Hospital; and to the Presbyterian Home for Aged Women, N. Y.
1854.	BYRNE, JOHN, M. D., Surgeon-in-Chief to St. Mary's Hospital for Women, Brooklyn, L. I. *Bul.* 1.
Original.	* CAMERON, JAMES, M. D., *Obit.* 12th December, 1851, *æt.* 66.
1862.	* CAMMANN, GEORGE P., M. D., late Physician to the Demilt Dispensary; Consulting Physician to St. Luke's Hospital, N. Y. *Obit.* 16th February, 1863, *æt.* 59.
Original.	* CAMPBELL, JAMES, M. D., *Obit.* 12th March, 1853, *æt.* 59.
1869.	CARO, SALVATORE, M. D. *Trans.* 1.
1858.	* CARRINGTON, WILLIAM A., M. D., *Obit.* 17th July, 1866.
Original.	* CARTER, GALEN, M. D., V. P. 1849–'52; Trust. 1851. *Obit.* 2d April, 1870, *æt.* 74.

ELECTED.

1869. CHADSEY, ALONZO J., M. D.

1847. CHALMERS, THOMAS C., M. D., Ex-surgeon to New York Hospital.

1862. CHAMBERLAIN, WILLIAM M., M. D., late Physician to the Demilt Dispensary, N. Y.; Physician to the Charity Hospital, N. Y.; R. S. 1865–'68.

Original. * CHAPIN, JOHN R., M. D., *Obit.* 23d June, 1852, *æt.* 41.

1872. CHAUVEAU, JEAN F., M. D.

1847. * CHEESMAN, JOHN C., M. D., late Surgeon to New York Hospital. *Obit.* 11th October, 1862, *æt.* 75.

1871. CHEESMAN, TIMOTHY MATLACK, M. D.

1865. * CHILDS, TIMOTHY, M. D., Professor of Anatomy in Bellevue Hospital Medical College. *Obit.* 3d September, 1865, *æt.* 42.

1856. CHURCH, ALLEN S., M. D. *Trans.* 1; *Bul.* 1.

1859. * CHURCHILL, CHARLES W., M. D., *Obit.* 27th October, 1859.

1856. CLARK, ALONZO, M. D., LL. D., President of, and Professor of Pathology and Practice of Medicine in the College of Physicians and Surgeons, N. Y.; Physician to Bellevue Hospital, N. Y. *Bul.* 2.

1870. CLARK, JAMES G., M. D., W. New Brighton, S. I., N. Y.

Original. CLARKSON, CORNELIUS V., M. D.

Original. CLEMENTS, JAMES W. G., M. D., Physician to the New York Institution for the Blind.

1859. * COCHRAN, GEORGE, M. D., Surgeon to the Brooklyn City Hospital, Brooklyn, L. I. *Obit.* 19th November, 1872, *æt.* 41.

1871. COLES, J. ACKERMAN, M. D., Scotch Plains, N. J.

1854. * CONANT, DAVID S., M. D., Professor of Surgery in the University of Vermont; Surgeon to the Demilt Dispensary, N. Y. *Obit.* 8th October, 1865, *æt.* 40. *Trans.* 1.

1864. * CONNOLLY, JAMES J., M. D., Physician to St. Vincent's Hospital, N. Y. *Obit.* 18th June, 1871, *æt.* 36.

Original. * COOPER, JAMES S., M. D., Physician to the Home for Aged and Respectable Indigent Females, N. Y. *Obit.* 11th April, 1867, *æt.* 49.

Original. * COVEL, JOHN C., M. D., Physician to the New York City Prison. *Obit.* 4th November, 1860, *æt.* 64.

1876. COWLES, EDWARD O., M. D., Visiting Physician to the New York Dispensary.

Elected.	
1862.	* Cox, Henry G., M. D., Professor of the Theory and Practice of Medicine in New York Medical College. *Obit.* 29th May, 1866, *æt.* 47.
1870.	Crampton, Henry E., M. D.
1849.	Crane, James, M. D., Physician to the Brooklyn City Hospital, Brooklyn, L. I.
1847.	Crane, John J., M. D., Surgeon to Bellevue Hospital, N. Y.
1866.	Crane, Joseph S., M. D., late Physician to the New York Lying-in Asylum.
1851.	* Creveling, Abraham, M. D., *Obit.* 28th April, 1853, *æt.* 39.
1868.	* Dalton, Edward B., M. D., late Surgeon of, and Medical Director of U. S. Volunteers. *Obit.* 13th May, 1872, *æt.* 37.
1856.	Dalton, John C., M. D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, N. Y.; Orator, 1873; V. P., now in office. *Trans.* 7.
1867.	Dana, Samuel W., M. D., late Physician to New York Dispensary. *Trans.* 1.
Original.	Davis, John, M. D.
Original.	* Delafield, Edward, M. D., President of, and Professor Emeritus of the College of Physicians and Surgeons, N. Y.; Consulting Physician to St. Luke's and New York State Woman's Hospitals, N. Y. *Obit.* Feb. 13, 1875, *æt.* 80.
1876.	De Luna, A. B., M. D., Physician to the Northern Dispensary.
1859.	Derby, Edward W., M. D., late Physician to the Eastern Dispensary, N. Y.
Original.	Detmold, William, M. D., Emeritus Professor of Military and Clinical Surgery in the College of Physicians and Surgeons, N. Y., Consulting Surgeon to the Presbyterian Hospital; V. P. 1853 to 1856; Orator, 1856. *Trans.* 1; *Bul.* 5; *Pro.* 1.
1860.	* Donaghe, William R., M. D., late Lecturer on Surgical Anatomy and Venereal Diseases in the University of the City of New York. *Obit.* 18th July, 1866, *æt.* 36.
1856.	Douglas, John H., M. D.
1848.	* Douglas, Robert, M. D., *Obit.* 25th July, 1861, *æt.* 47.
Original.	Downs, Henry S., M. D.

ELECTED.

Original. * DRAKE, BENJAMIN, M. D., late President New York County Medical Society, C. S. 1847. *Obit.* 11th January, 1871, *æt.* 65.

1858. DRAPER, WILLIAM H., M. D., Clinical Professor of the Diseases of the Skin in the College of Physicians and Surgeons, N. Y.; Physician to Roosevelt Hospital, N. Y. *Bul.* 6.

1847. DUDLEY, WILLIAM H., M. D., Consulting Physician to the Long Island College Hospital, Brooklyn, L. I.

1847. * DWIGHT, WILLIAM W., M. D., *Obit.* 11th July, 1861, *æt.* 54.

1847. * EARLE, EDWARD, M. D., *Obit.* 21st August, 1849, *æt.* 37.

1851. * ELDER, ALEXANDER, M. D., late Physician to the Demilt Dispensary, N. Y. *Obit.* 3d February, 1875, *æt.* 70.

1858. ELIOT, ELLSWORTH, M. D., late Vice-President N. Y. State Medical Society; late President of the New York County Medical Society.

1858. * ELLIOT, GEORGE T., M. D., Professor of Obstetrics in Bellevue Hospital Medical College; Physician to Bellevue Hospital. *Obit.* 28th January, 1871, *æt.* 43. *Trans.* 1; *Bul.* 2.

1870. * ELLIS, HENRY A., M. D., *Obit.* 25th January, 1876.

Original. * ELLIS, SAMUEL C., M. D. *Obit.* 20th July, 1874, *æt.* 71.

1872. ELSBERG, LOUIS, M. D., late Clinical Professor of the Diseases of the Throat in the University of the City of New York; Physician to Charity Hospital. *Trans.* 2.

1856. EMMET, THOMAS ADDIS, M. D., Surgeon to the New York State Woman's Hospital, N. Y.

Original. * ENOS, DEWITT C., M. D., Professor of Anatomy in the Long Island Hospital College; Surgeon to the Brooklyn City Hospital, Brooklyn, L. I. *Obit.* 14th December, 1868, *æt.* 48.

1867. FARLEY, JAMES L., M. D., Brooklyn, New York.

1864. FARNHAM, HORACE P., M. D., late Physician to the Northern Dispensary, N. Y.

Original. * FERGUSON, JOHN T., M. D., *Obit.* 11th October, 1859, *æt.* 55.

1847. * FERRIS, FLOYD T., M. D., *Obit.* 6th November, 1855, *æt.* 62.

1855. FINNELL, THOMAS C., M. D., late Demonstrator of Anat-

ELECTED.

omy in the University of the City of New York; Surgeon to St. Vincent's Hospital, N. Y. *Bul.* 1.

1847. * FISK, LYMAN, M. D., *Obit.* 1st August, 1859, *æt.* 36.

1847. FITCH, JAMES D., M. D., Consulting Physician to the Colored Home for Indigent and Aged, N. Y.

1862. FLINT, AUSTIN, M. D., Professor of the Principles and Practice of Medicine and Clinical Medicine in Bellevue Hospital Medical College; Physician to Bellevue Hospital, N. Y.; Orator 1868; V. P. 1871 and 1872; P. 1873 and 1874; Trust. 1875, and now in office. *Trans.* 2; *Bul.* 3.

1862. FLINT, AUSTIN, Jr., M. D., Professor of Physiology in Bellevue Hospital Medical College; late Physician to Bellevue Hospital, N. Y.

1870. FOSTER, FRANK P., M. D., Physician to the New York Dispensary, N. Y. *Trans.* 1.

Original. FOSTER, JOEL, M. D., V. P. 1859 to 1862; Trust. 1862 to 1866.

Original. * FOSTER, SAMUEL CONANT, M. D., late Physician to Bellevue Hospital, N. Y.; R. S. 1855 and 1856; V. P. 1858 and 1859; Orator 1861 and 1862. *Obit.* 18th April, 1873, *æt.* 57. *Trans.* 1.

1874. FOWLER, GEORGE B., M. D., Examiner in Physiology, College of Physicians and Surgeons, N. Y.; Surgeon to the New York Dispensary. *Trans.* 1.

1856. * FOY, MICHAEL E., M. D., Surgeon of the 38th Regiment New York Volunteers. *Obit.* 9th June, 1861.

Original. * FRANCIS, JOHN W., M. D., late Professor of Obstetrics and Medical Jurisprudence in Rutgers Medical College, N. Y.; Orator 1847; V. P. 1847; P. 1848. *Obit.* 8th February, 1861, *æt.* 72.

1863. FRANCIS, SAMUEL W., M. D., Newport, R. I.

1871. FRANKEL, EDWARD, M. D., Physician to Charity Hospital, Physician to the New York and Eastern Dispensaries, N. Y.

1864. FREEMAN, N. MARSTON, M. D., late Physician to the Yorkville Dispensary, New York.

1870. FROTHINGHAM, WILLIAM, M. D.

1871. FULLER, ROBERT M., M. D.

1865. FURMAN, G., M. D.

1869. GALLATIN, ALBERT H., M. D., Professor of Analytical Chemistry in Cooper Institute, N. Y. *Trans.* 1.

ELECTED.	
Original.	GARRISH, JOHN P., M. D., late Physician to the New York Ophthalmic Hospital, N. Y.
1870.	GAY, HARVEY S., M. D., late Physician to the New York Lying-in Asylum.
1848.	GESCHEIDT, ANTHONY, M. D., Hastings-on-the-Hudson, N. Y.
1863.	GILFILLAN, WILLIAM, M.D., Professor of Therapeutics and Materia Medica in Long Island Hospital Medical College, Brooklyn, L. I.
1847.	* GILFORD, JACOB T., M. D., *Obit.* 11th June, 1869, *æt.* 63.
1856.	GOMEZ, HORATIO, M. D., late Physician to the New York Dispensary, N. Y.
1856.	GOULEY, JOHN WM. S., M. D., Professor of Diseases of the Genito-Urinary System, in the University of the City of New York; Surgeon to Bellevue Hospital, N. Y.
1847.	* GRAHAM, JOHN, M. D., *Obit.* 20th May, 1847, *æt.* 47.
1848.	* GREEN, DAVID, M. D., *Obit.* 18th October, 1856, *æt.* 60.
Original.	* GREEN, HORACE, M. D., Professor of the Theory and Practice of Medicine in New York Medical College. *Obit.* 29th November, 1866, *æt.* 63. *Trans.* 1.
Original.	* GREENE, ISAAC, M. D., *Obit.* 2d July, 1854, *æt.* 38.
Original.	* GRISCOM, JOHN H., M. D., late Physician to New York Hospital; Orator 1854; V. P. 1854. *Obit.* 28th April, 1874, *æt.* 64. *Trans.* 1; *Bul.* 4.
1872.	GRISWOLD, HENRY, M. D.
1847.	* GUERNSEY, PETER B., M. D., Croton Falls, N. Y. *Obit.* 26th November, 1873, *æt.* 69.
1847.	* GUNN, ALEXANDER N., M. D., late Health Officer of the Port of New York. *Obit.* 20th December, 1871, *æt.* 60.
1867.	HACKLEY, CHARLES E., M. D., Physician to New York Hospital.
1875.	HADDEN, ALEXANDER, M. D., Physician to Presbyterian Hospital, N. Y.
Original.	HALL, EDWARDS, M. D.
Original.	HALL, SAMUEL, M. D., late Physician to the New York Dispensary.
1874.	HALL, W. H., M. D.
Original.	* HALSTED, JONATHAN, M. D., *Obit.* 10th April, 1856, *æt.* 39.

ELECTED.

1873. HAMILTON, ALLAN McL., Lecturer on Diseases of Nervous System in the Long Island College Hospital. *Trans.* 1.

1864. HAMILTON, FRANK H., M. D., late Professor of Military Surgery in Bellevue Hospital Medical College; Surgeon to Bellevue Hospital, N. Y. *Bul.* 2.

1874. HANKS, HORACE T., M. D., Physician to the Demilt Dispensary, New York; A. S., now in office.

1857. HARRIS, ELISHA, M. D., late Registrar of Vital Statistics of the Health Department of the City of New York. *Bul.* 3; *Pro.* 1.

1859. * HARSEN, JACOB, M. D., Trust. 1862. *Obit.* 31st December, 1862, *æt.* 55.

Original. * HART, JOHN, M. D., *Obit.* 9th August, 1867, *æt.* 57.

1865. * HAZLETT, JOHN, M. D., *Obit.* 4th March, 1870, *æt.* 53.

1865. HEDGES, DAVID A., M. D., Consulting Physician of the Northwestern Dispensary, N. Y.

1871. HENRY, MORRIS H., M. D., Surgeon-in-Chief to the New York State Emigrants' Hospital, N. Y.

1847. * HENSCHEL, CHARLES, M. D., late President of the New York Obstetrical Society. *Obit.* 18th September, 1872, *æt.* 63.

1867. HERRICK, EVERETT, M. D.

1857. HERZOG, MAX, M. D., late Physician to the German Dispensary, N. Y. *Bul.* 1.

1863. * HEWIT, HENRY S., M. D., Lecturer on Surgery in the University of the City of New York; Surgeon to Charity Hospital, N. Y. *Obit.* 19th August, 1873, *æt.* 47. *Bul.* 1.

1856. HEYWOOD, CHARLES F., M. D., late Physician to St. Luke's Hospital, N. Y.; R. S. 1857 and 1858.

1856. HINTON, JOHN H., M. D., late Lecturer on Surgery in the University of the City of New York; Surgeon to the Presbyterian Hospital; R. S. 1861 to 1865; L., now in office.

1854. HIRSCH, SIMON, M. D.

Original. * HOBART, WILLIAM H., M. D., *Obit.* 21st Jan., 1857, *æt.* 53.

1847. * HOGAN, DANIEL M., M. D., *Obit.* 1849.

1871. HOGAN, EDWARD J., M. D.

1867. HOGAN, MICHAEL, M. D.

1854. HOLCOMBE, WILLIAM F., M. D., A. S. 1856.

Original. * HORSFIELD, THOMAS W., M. D., *Obit.* 19th Feb., 1868, *æt.* 64.

ELECTED.

1866. HOWARD, BENJAMIN, M. D., late Professor of Clinical and Operative Surgery in Long Island Hospital College; Professor of Diseases of the Genito-Urinary Organs in the University of Vermont.

1872. HOWE, JOSEPH W., M. D., Clinical Professor of Surgery in the University of the City of New York; Surgeon to Charity Hospital, N. Y.

Original. HUBBARD, SAMUEL T., M. D., Physician to the Presbyterian Hospital; late President New York County Medical Society; V. P. 1873 to 1876; C. S. 1853 to 1858; Trust. 1862 to 1873, 1876, now in office.

1874. HUDSON, E. DARWIN, Jr., M. D., Professor of Principles and Practice of Medicine in the Woman's Medical College, N. Y. Orator, 1875. *Trans.* 1.

1866. HULL, JOSEPH J., M. D., late Surgeon to St. Luke's Hospital, N. Y.; Physician to the Nursery and Child's Hospital, N. Y.

1867. HUMPHREYS, GEORGE H., M. D.

1849. * HUNTER, ABRAHAM T., M. D., *Obit.* 1st August, 1849, *æt.* 52.

1866. HUSTED, NATHANIEL C., M. D.

Original. * HUTCHINSON, EUGENE F., M. D., Physician to the New York Dispensary. *Obit.* 2d March, 1848, *æt.* 24.

1857. HUTCHISON, JOSEPH C., M. D., late Professor of Operative Surgery in Long Island Hospital College; Surgeon to Brooklyn City Hospital, Brooklyn, L. I.; V. P. 1869 to 1873. *Trans.* 1.

1848. * HYSLOP, JAMES, M. D., *Obit.* 17th May, 1870, *æt.* 53.

1850. * ISAACS, CHARLES E., M. D., Brooklyn, L. I., V. P. 1858. *Obit.* 16th June, 1860, *æt.* 48. *Trans.* 4.

1875. IVES, FRANK L., M. D.

Original. * IVES, GEORGE W., M. D., *Obit.* 6th December, 1874, *æt.* 55.

Original. IVES, JOHN, M. D.

Original. JACKSON, WILLIAM H., M. D.

1857. JACOBI, ABRAHAM, M. D., Clinical Professor of the Diseases of Children in the College of Physicians and Surgeons, N. Y.; Physician to the German and Mount Sinai Hospitals, N. Y. *Bul.* 1.

1853. JANES, EDWARD H., M. D., Professor of Hygiene in the Woman's Medical College, N. Y.; Assistant Sanitary

ELECTED.

Superintendent of the Health Department of the City of New York; R. S. 1868 to 1871.

1867. JANVRIN, JOSEPH E., M. D., Assistant-Surgeon to the Woman's Hospital of the State of N. Y.

1852. JENKINS, J. FOSTER, M. D., Consulting Physician to St. John's Riverside Hospital, Yonkers, N. Y.

1855. JOHNSON, LAURENCE, M. D., Physician to the Demilt Dispensary.

1855. * JOHNSON, WILLIAM J., M. D., *Obit.* 22d September, 1860, *æt.* 55.

Original. * JOHNSTON, FRANK U., M. D., Physician to the New York Hospital; V. P. 1847. *Obit.* 7th January, 1858, *æt.* 61.

1851. JONES, ALANSON S., M. D., late Surgeon of Police.

1856. * JONES, E. LEE, M. D., *Obit.* 30th January, 1876, *æt.* 46.

1847. JONES, WILLIAM W., M. D., late Physician to St. Luke's Hospital, N. Y.

1872. JUDSON, ADONIRAM B., A. M., M. D., Assistant Surgeon to the New York Orthopedic Dispensary and Hospital. *Trans.* 1.

1857. * KAMMERER, JOSEPH, M. D., Professor of Diseases of Women and Children in the University of the City of New York. *Trans.* 1; *Bul.* 2. *Obit.* 10th June, 1875, *æt.* 53.

1876. KATZENBACH, W. H., M. D., Physician to Bellevue Hospital Dispensary, N. Y.

Original. * KEARNEY, RAVAUD, M. D., *Obit.* 21st March, 1849, *æt.* 26.

Original. KEENE, STEPHEN S., M. D., Providence, R. I.

1847. KENNEDY, JAMES, M. D., late Vice-President of the New York County Medical Society.

1863. KERRIGAN, JOSEPH A., M. D., Surgeon to St. Vincent Hospital, N. Y.

Original. KILBOURNE, J. SAGE, M. D.

1854. * KIMBARK, EVERETT H., M. D., *Obit.* 29th August, 1872, *æt.* 53.

1851. * KINGSBURY, GEORGE H., M. D., *Obit.* 4th May, 1852, *æt.* 31.

Original. * KISSAM, RICHARD S., M. D., late Professor of Surgery in Castleton Medical College, Vt.; Trust. 1853 to 1860. *Obit.* 28th November, 1861, *æt.* 53.

1874. KNAPP, HERMAN, M. D., Surgeon in Charge of the New York Ophthalmic and Aural Institute; Lecturer on

ELECTED.

Diseases of the Eye and Ear in the College of Physicians and Surgeons, N. Y.

1876. KNIGHT, JAMES, M. D., Resident Physician and Surgeon to the Hospital for the Relief of Ruptured and Crippled, N. Y.

1854. * KRACKOWIZER, ERNST, M. D., Surgeon to the New York Hospital; Surgeon to the German Hospital, N. Y. *Trans.* 2; *Bul.* 2. *Obit.* 23d September, 1875, *æt.* 53.

1865. LAWRENCE, JONATHAN S., M. D.

1869. LEALE, CHARLES A., M. D., late Surgeon in Charge of Officers' Wards, and Executive Officer U. S. Army General Hospital, Washington; late Physician to the Northwestern Dispensary, N. Y. *Trans.* 3.

1854. LEAMING, JAMES R., M. D., Emeritus Professor of Principles and Practice of Medicine in the Woman's Medical College, N. Y.; Physician to St. Luke's Hospital, N. Y. *Trans.* 2; *Bul.* 3.

1869. LEE, CHARLES C., M. D., Surgeon to Charity Hospital, N. Y.

1874. LEFFERTS, GEORGE M., M. D., Physician to the Demilt Dispensary, N. Y.; Clinical Professor of Laryngoscopy and Diseases of the Throat in College of Physicians and Surgeons, N. Y.

1872. LEO, SIMEON N., M. D., Physician to the Home for Aged Hebrews, N. Y.

1850. * LEO WOLFE, GEORGE, M. D., *Obit.* 14th March, 1855, *æt.* 40.

Original. LEO WOLFE, MORRIS, M. D.

1851. * LEVERIDGE, BENJAMIN C., M. D., *Obit.* 16th April, 1862, *æt.* 63.

1856. LIDELL, JOHN A., M. D., late Surgeon to Bellevue Hospital, N. Y. *Trans.* 1.

Original. LINSLY, JARED, M. D., Consulting Physician to the Presbyterian Hospital, New York Lying-in Asylum, and to the New York Dispensary. Trust. 1865 to 1870.

1864. LITTLE, JAMES L., M. D., Lecturer on Operative Surgery in the College of Physicians and Surgeons, N. Y.; Surgeon to St. Luke's and St. Vincent's Hospitals, N. Y.

1855. LIVINGSTON, WATTS C., M. D., late Physician to Demilt Dispensary, N. Y.

1876. LOCKROW, A. V. B., M. D., Physician to Demilt Dispensary, N. Y.

1861. * LOINES, JONAS P., M. D., Vaccine Physician to the East-

ELECTED.

ern Dispensary, N. Y. *Obit.* 15th December, 1873, *æt.* 52.

1863. LOOMIS, ALFRED L., M. D., Professor of Pathology and Practice of Medicine in the University of the City of New York; Physician to Bellevue Hospital, N. Y. *Trans.* 1; *Bul.* 3.

1876. LORDLY, J. E. M., M. D.

1869. LORING, EDWARD G., M. D., Consulting Surgeon to St. Luke's and the Brooklyn Eye and Ear Hospitals; Surgeon to the New York Eye and Ear Infirmary. *Trans.* 1.

1876. LUDLUM, W. S., M. D., Physician to Demilt Dispensary.

1871. LUSK, WILLIAM T., M. D., Professor of Obstetrics and the Diseases of Women and Clinical Midwifery in Bellevue Hospital Medical College, N. Y.; Physician to the Bellevue Hospital, N. Y.

1875. LYNCH, PATRICK J., M. D.

1847. * LYON, JAMES L., M. D., *Obit.* 24th December, 1858, *æt.* 50.

1864. MACGREGOR, JAMES R., M. D.

1857. * MCALLISTER, GEORGE, M. D., *Obit.* 29th July, 1864, *æt.* 37.

1866. MCCLELLAN, CHRISTOPHER R., M. D., Brooklyn, L. I.

Original. * MCCLELLAND, JOHN, M. D., late Physician to the New York City Lunatic Asylum. *Obit.* 20th February, 1875, *æt.* 69.

Original. MCCREADY, BENJAMIN W., M. D., late Professor of Materia Medica in Bellevue Hospital Medical College; Consulting Physician to Bellevue Hospital, N. Y.

1847. * MCDONALD, JAMES, M. D., late Physician to Bloomingdale Lunatic Asylum, N. Y. *Obit.* 5th May, 1859, *æt.* 45.

1876. MCLANE, J. W., M. D., Adjunct Professor of Obstetrics to College of Physicians and Surgeons, N. Y.; Physician to St. Luke's Hospital, N. Y.

1857. MCLEOD, S. B. WYLIE, M. D., late Physician to the New York Lying-in Asylum.

1865. MCMILLAN, CHARLES, M. D., late Physician to the Orphan Asylum of the Protestant Episcopal Church, N. Y.

1847. * MACNEVEN, WILLIAM H., M. D., late Physician to the New York Dispensary, N. Y. *Obit.* 12th May, 1854, *æt.* 38.

1848. MCNULTY, JOHN, M. D., late Surgeon U. S. Volunteers. *Bul.* 1.

Original. * MANLEY, JAMES R., M. D., late Lecturer on Obstetrics in

ELECTED.

the College of Physicians and Surgeons, N. Y.; V. P. 1849; Orator 1848. *Obit.* 21st November, 1857, *æt.* 70.

1876. MANN, M. D., M. D., Physician to the New York Dispensary; Lecturer on the Microscope as an Aid to Diagnosis in the College of Physicians and Surgeons, N. Y.

Original. MARKOE, THOMAS M., M. D., Professor of Surgery in the College of Physicians and Surgeons, N. Y.; Surgeon to Bellevue and Roosevelt Hospitals, N. Y.; L. 1847. *Bul.* 1.

1847. * MARTIN, JOSEPH, M. D., *Obit.* 26th April, 1864, *æt.* 67. *Bul.* 2.

1872. MARTIN, T. DWIGHT, M. D.

Original. * MARVIN, DAVID D., M. D., *Obit.* 21st October, 1852, *æt.* 40.

1872. MASON, JOHN J., M. D., Physician to Hospital for Epileptics and Paralytics, Blackwell's Island, N. Y.

1847. MAXWELL, WILLIAM H., M. D., Consulting Surgeon to the New York Dispensary.

Original. * MEIKLEHAM, DAVID S., M. D., *Obit.* 20th November, 1849, *æt.* 45.

1874. METCALFE, FRANCIS J., M. D., Physician to New York Dispensary.

Original. METCALFE, JOHN T., M. D., Emeritus Professor of Clinical Medicine in the College of Physicians and Surgeons, N. Y.; Consulting Physician to Bellevue, St. Luke's, and Roosevelt Hospitals. *Trans.* 1; *Bul.* 2.

1848. * MILLER, JOHN, M. D., *Obit.* 13th January, 1863, *æt.* 56.

1848. * MILLER, WM. ELLISON, M. D., *Obit.* 16th January, 1852, *æt.* 52.

1848. * MINER, WILLIAM, M. D., *Obit.* 16th November, 1859, *æt.* 45.

Original. * MINER, WILLIAM W., M. D., V. P. 1848. *Obit.* 20th March, 1863, *æt.* 83.

1847. MITCHELL, CHAUNCEY L., M. D., late Professor of Obstetrics in Castleton Medical College, Vt.; Consulting Physician to St. Mary's Hospital. Brooklyn, L. I.

1852. MONELL, JOSEPH A., M. D.

1871. MONELL, JOSEPH S., M. D.

1848. * MOORE, SAMUEL W., M. D., late Physician to the New York Hospital. *Obit.* 25th August, 1854, *æt.* 68.

1849. * MORAN, THOMAS, M. D., *Obit.* 1853.

ELECTED.

1870. MORRIS, MOREAU, M. D., late Sanitary Inspector of the Health Department of the City of New York.

1870. MORRIS, STUYVESANT F., M. D., Physician to the New York Dispensary.

1869. MORTON, JEREMIAH C., M. D., Physician to the Northern Dispensary.

1874. MOSHER, JACOB S., M. D., Albany, N. Y.

Original. * MOTT, VALENTINE, M. D., LL. D., Professor of Surgery in the University of the City of New York; Consulting Surgeon to New York and Bellevue Hospitals; P. 1849 and 1857. *Obit.* 26th April, 1865, *æt.* 79. *Trans.* 4.

1875. MUNDÉ, PAUL F., M. D., Assistant Surgeon to the Woman's Hospital, N. Y.

1871. NEFTEL, WILLIAM B., M. D.

1848. * NEILSON, JOHN, M. D., *Obit.* 19th June, 1857, *æt.* 82.

1852. * NELSON, JAMES B., M. D., *Obit.* 28th September, 1874, *æt.* 61.

1874. NESMITH, ROBERT D., M. D.

1847. NICHOLS, ELIAS S., M. D.

1859. NICHOLS, TRUMAN, M. D.

1873. NICOLL, HENRY D., M. D., Physician to the Presbyterian Home for Aged Women, N. Y.

1861. NOEGGERATH, EMIL, M. D., late Professor of Clinical Midwifery in New York Medical College; Physician to the German Hospital, N. Y. *Bul.* 2.

1871. * NOTT, JOSIAH CLARK, M. D., late Professor of Surgery in Mobile Medical College. *Obit.* 31st March, 1873, *æt.* 69.

1862. NOYES, HENRY D., M. D., Professor of Ophthalmology and Otology in Bellevue Hospital Medical College, N. Y.; Surgeon to the New York Eye and Ear Infirmary. *Bul.* 2.

Original. * OGDEN, BENJAMIN, M. D., late Physician to the Bloomingdale Asylum for the Insane; Consulting Physician to St. Luke's Hospital, N. Y.; Trust. 1853 to 1859, 1861. *Obit.* 18th June, 1867, *æt.* 69.

1862. ORDRONAUX, JOHN, M. D., late Professor of Medical Jurisprudence in Law School of Columbia College; Orator 1865.

1873. ORTON, SAMUEL H., M. D.

1861. OTIS, FESSENDEN N., M. D., Clinical Professor of Venereal

ELECTED.

Diseases in the College of Physicians and Surgeons, N. Y.; Surgeon to Charity Hospital, N. Y.

1870. O'MEAGHER, WILLIAM, M. D., late Physician to the New York Dispensary.

1857. *O'REILLY, JOHN, M. D., *Obit.* 6th December, 1868, *æt.* 55. *Trans.* 1.

1855. O'SULLIVAN, RICHARD J., M. D., Consulting Physician to the Eastern Dispensary, N. Y.

1871. PACKARD, CHARLES W., M. D., Physician to St. Luke's Hospital, N. Y.

1864. PAINE, MARTYN, A. M., M. D., LL. D., Emeritus Professor of the Institutes of Medicine, Materia Medica, and Therapeutics, in the University of the City of New York.

1851. PALMER, LUCIUS N., M. D., Physician to the Brooklyn Dispensary and Hospital, E. D. Brooklyn, L. I.

1869. PARDEE, CHARLES I., M. D., Professor of Diseases of the Ear in the Medical Department of the University of the City of New York; Surgeon to the Manhattan Eye and Ear Hospital, N. Y.

1873. PARIGOT, JULIUS, M. D. *Bul.* 2.

Original. PARKER, WILLARD, M. D., Professor of Clinical Surgery in the College of Physicians and Surgeons, N. Y.; Consulting Surgeon to the New York, Bellevue, and Roosevelt Hospitals, N. Y.; Trust. 1851; V. P. 1853; P. 1856. *Bul.* 2.

1874. PARKER, WILLARD, Jr., M. D.

1847. *PARKINSON, WILLIAM B., M. D., Physician to the New York Dispensary. *Obit.* 11th May, 1856, *æt.* 45.

1847. *PAUL, JAMES C., M. D., *Obit.* 5th May, 1859.

1858. PEASLEE, EDMUND R., M. D., LL. D., Professor of Gynæcology in Bellevue Hospital Medical College; late Professor of Anatomy in the New York Medical College; late Professor of the Diseases of Women in Albany Medical College, N. Y.; Surgeon to the New York State Woman's Hospital, N. Y.; Orator 1858; V. P. 1868 to 1871; P. 1871 and 1872; Trus. 1873, now in office. *Trans.* 4; *Bul.* 5.

Original. *PENNELL, RICHARD, M. D., *Obit.* 11th April, 1861, *æt.* 62.

1850. PETERS, GEORGE A., M. D., Surgeon to the New York and St. Luke's Hospitals, N. Y.; A. S. 1852.

1870. PETERS, JOHN C., M. D., late President of the Medical

ELECTED.

Journal Association of the City of New York; President of the Neurological Society. *Trans.* 1.

1874. PEUGNET, EUGENE, M. D.

Original. * PHILLIPS, SAMUEL B., *Obit.* 3d March, 1857, *æt.* 54.

Original. * PIATT, WILLIAM F., M. D., *Obit.* 6th May, 1848, *æt.* 42.

1875. PIFFARD, HENRY G., A. M., M. D., Professor of Dermatology in the University of the City of New York; Surgeon to the Charity Hospital, and to the New York Dispensary for the Diseases of the Skin, etc., etc. *Trans.* 1.

1867. PINCKNEY, HOWARD, M. D., Assistant Surgeon to the New York Eye and Ear Infirmary, N. Y.

1873. POLK, WILLIAM M., M. D., Professor of Materia Medica and Therapeutics and Clinical Medicine in Bellevue Hospital Medical College; Physician to Bellevue Hospital, N. Y.

1865. POMEROY, OREN D., M. D., Surgeon to the Manhattan Eye and Ear Hospital, N. Y.; late Physician to the Northern Dispensary, N. Y.

Original. POND, JAMES O., M. D., T., since 1848, now in office.

1867. POOLEY, J. H., JR., M. D., Professor of Surgery in Starling Medical College, Ohio.

1869. POOLEY, THOMAS R., M. D., Surgeon to Charity Hospital, N. Y., Assistant-Surgeon to the New York Ophthalmic and Aural Institute.

1856. * PORTER, MORTIMER G., M. D., *Obit.* 24th November, 1863, *æt.* 37. *Bul.* 1.

1847. POST, ALFRED C., M. D., President of the Medical Faculty, and Professor Emeritus of Clinical Surgery in the University of the City of New York; Consulting Surgeon to the New York Hospital; Surgeon to the Presbyterian Hospital; Orator 1849; V. P. 1861 to 1866; P. 1867–1868; *Bul.* 4; *Pro.* 1.

1870. POST, WILLIAM, H. B., M. D., Sanitary Inspector, Health Department of the City of New York; A. S. 1871 to 1874.

Original. * POWER, WILLIAM, M. D., *Obit.* 14th September, 1858, *æt.* 60.

Original. * PRATT, PETER, M. D., *Obit.* 1860, *æt.* 52.

1861. PRINCE, CHRISTOPHER, M. D., late Surgeon New York Police. *Bul.* 2.

1871. PURDY, ALFRED E. M., M. D., late Surgeon New York Police.

ELECTED.

Original. PURDY, ALFRED S., M. D.

Original. PURDY, SAMUEL A., M. D., R. S. 1853 and 1854.

Original. PURPLE, SAMUEL S., M. D., Honorary Member of the Medical Society of the State of New York; late Physician to the New York Dispensary; V. P. 1872 to 1874, P. 1875, now in office. *Trans.* 1; *Pro.* 1.

1847. PUTNAM, FREDERICK A., M. D.

1876. QUACKENBOS, H. F., M. D.

1876. RABORG, S. A., M. D., House Surgeon to Central Dispensary, N. Y.

1875. RAMSDELL, EDWIN D., M. D.

1851. RANDOLPH, ISRAEL, M. D.

1863. RANNEY, HENRY D., M. D.

1859. RANNEY, LAFAYETTE, M. D.

1851. *RANNEY, MOSES H., Physician to the New York City Lunatic Asylum. *Obit.* 7th December, 1864, *æt.* 50. *Bul.* 1.

1856. RAPHAEL, BENJAMIN I., M. D., late Professor of Surgery in New York Medical College; Surgeon to Mount Sinai Hospital, N. Y.

1859. *RAY, ROBERT, Jr., M. D., *Obit.* 3d July, 1860, *æt.* 27.

Original. *REESE, DAVID MEREDITH, M. D., late Professor of the Principles of Surgery in Castleton Medical College. *Obit.* 13th May, 1861, *æt.* 60.

1872. REINFELDER, MAX J., M. D., Physician to St. John's Riverside Hospital, Yonkers, N. Y.

1866. REYNOLDS, JAMES B., M. D., Physician to the Nursery and Child's Hospital, N. Y.

1855. RICHARDS, JOSEPH W., M. D.

1874. ROBERT, CHAS. S., M. D.

1874. ROBERTS, NATHAN S., M. D.

Original. *ROBERTS, WILLIAM C., late Physician to the Northern Dispensary, N. Y.; Orator 1859, V. P. 1870 to 1873. *Obit.* 9th December, 1873, *æt.* 63. *Bul.* 6.

Original. *ROBESON, ABEL B., Physician to Bellevue Hospital, N. Y. *Obit.* 22d March, 1853, *æt.* 36.

1872. ROBIE, JOHN W., M. D., Physician to the Masonic Board of Relief, New York.

1869. ROCKWELL, ALPHONZO D., M. D. *Bul.* 1.

ELECTED.

Original. ROCKWELL, WILLIAM, M. D., *Obit.* 30th December, 1867, *æt.* 67.

1873. * RODENSTEIN, CHARLES F., M. D., Physician to the New York Catholic Protectory, Fordham, N. Y., *Obit.* 18th March, 1876, *æt.* 49.

1862. RODENSTEIN, LOUIS A., M. D.

Original. * RODGERS, JOHN KEARNY, M. D., Surgeon to the New York Hospital; V. P. 1848 to 1851; Trust. 1851. *Obit.* 9th November, 1851, *æt.* 59.

1847. * ROGERS, J. SMYTH, M. D., late Professor of Materia Medica in the New York College of Pharmacy. *Obit.* 29th March, 1851, *æt.* 57.

1872. ROOF, STEPHEN W., M. D.

1865. ROOSA, D. B. ST. JOHN, M. D., Professor of Ophthalmology and Otology in the University of the City of New York; Surgeon to the Manhattan Eye and Ear Hospital, N. Y.; Orator 1874. *Bul.* 1.

1862. ROSENBERG, EMIL, M. D., Physician to the German Dispensary, N. Y.

1858. ROSS, JAMES, M. D., late Physician to the Northern Dispensary, N. Y.

1871. RUSSEL, CHARLES P., M. D., Sanitary Inspector of the Health Department of the City of New York. *Trans.* 2.

Original. SABINE, GUSTAVUS A., M. D., Consulting Physician to the New York State Woman's Hospital, N. Y.

1874. SABINE, THOS. T., M. D., Adjunct Professor of Anatomy College of Physicians and Surgeons of New York; Surgeon to St. Luke's Hospital, N. Y.

1873. SATTERLEE, F. LE ROY, M. D., Professor of Chemistry, Materia Medica, and Therapeutics in the New York College of Dentistry.

1864. SATTERLEE, RICHARD S., M. D., Brigadier-General U. S. Army.

Original. SAYRE, LEWIS A., M. D., Professor of Orthopedic Surgery Fractures and Dislocations, and Clinical Surgery, in Bellevue Hospital Medical College, N. Y.; Surgeon to Bellevue Hospital, N. Y. *Trans.* 1; *Bul.* 2.

1852. * SCHILLING, ERNST, M. D., Physician to New York State Emigrant Hospital, N. Y. *Obit.* 25th April, 1872, *æt.* 62.

Original. * SCHMIDT, JOHN W., Jr., M. D., Surgeon to St. Vincent's Hospital, N. Y. *Obit.* 1857, *æt.* 50.

1847. SCHRIMER, WILLIAM, M. D.

ELECTED.

1874. Schultze, Louis F., M. D.

1873. Seguin, Edward C., M. D., Physician to the Hospital for Epileptics and Paralytics; Clinical Professor of Diseases of the Mind and Nervous System in the College of Physicians and Surgeons, N. Y. *Trans.* 1.

1870. Sell, E. H. M., M. D., Physician to the Northwestern Dispensary, N. Y.

1856. * Sewall, John G., M. D., Physician to the Northwestern Dispensary, N. Y. *Obit.* 18th January, 1874, *æt.* 51.

Original. * Shanks, John, M. D., *Obit.* 10th August, 1870, *æt.* 69.

1862. * Sheppard, John W., M. D., *Obit.* 5th October, 1868, *æt.* 46.

Original. * Sherwood, Burritt, M. D., *Obit.* 10th August, 1854, *æt.* 53.

1856. Sims, J. Marion, M. D., Orator 1857. *Bul.* 2.

1876. Smith, A. A., M. D., Physician to the Demilt Dispensary N. Y.; Lecturer Adjunct upon Clinical Medicine in Bellevue Hospital Medical College, N. Y.

Original. * Smith, David, M. D., *Obit.* 16th January, 1867, *æt.* 57.

1870. * Smith, David A., M. D., *Obit.* 9th April, 1872, *æt.* 28.

Original. * Smith, Gilbert, M. D., *Obit.* 16th July, 1851, *æt.* 80.

1858. Smith, Gouverneur M., M. D., Physician to the New York and Presbyterian Hospitals; L. 1861, 1862; Orator 1869; V. P. 1875, now in office. *Trans.* 5; *Bul.* 3.

1864. Smith, Hanbury, M. D.

1853. Smith, James O., M. D.

1867. Smith, Jerome C., M. D., late Physician to the Northeastern Dispensary, N. Y.

Original. * Smith, Joseph Mather, M. D., Professor of the Theory and Practice of Medicine, and subsequently of Materia Medica and Clinical Medicine in the College of Physicians and Surgeons, N. Y., 1826–1866; Physician to New York Hospital; Orator 1850; V. P. 1850 and 1851; P. 1854. *Obit.* 22d April, 1866, *æt.* 77. *Bul.* 2.

1856. Smith, J. Lewis, M. D., Physician to Charity and Infant Hospitals, N. Y. *Trans.* 2.

1866. Smith, Oscar G., M. D., Physician to Nothern Dispensary.

1855. Smith, Stephen, M. D., Professor of Orthopedic Surgery and Surgical Jurisprudence in the University of the City of New York; late Professor of Anatomy in

ELECTED.

Bellevue Hospital Medical College, N. Y.; Surgeon to Bellevue Hospital; Orator 1867. *Bul.* 1.

1870. SNELLING, FREDERICK G., M. D.

1847. * SNOWDEN, JOHN, M. D., *Obit.* 22d January, 1848.

1864. SPEIR, S. FLEET, M. D., Surgeon to the Brooklyn City Hospital, Brooklyn, L. I.

Original. * SPRING, EDWARD, M. D., *Obit.* 13th February, 1850, *æt.* 51.

1859. SQUIBB, EDWARD R., M. D., Brooklyn, L. I. *Bul.* 4.

1876. STANLEY, C. GRAHAM, M. D.

Original. * STEARNS, JOHN, M. D., P. 1847. *Obit.* 17th March, 1848, *æt.* 78.

Original. * STEPHENSON, MARK, M. D., Physician to the Ophthalmic Hospital, N. Y. *Obit.* 28th August, 1865, *æt.* 62.

Original. * STEVENS, ALEXANDER H., M. D., LL. D., Emeritus Professor of Surgery in the College of Physicians and Surgeons, N. Y.; Consulting Surgeon to New York Hospital; P. 1851. *Obit.* 30th March, 1869, *æt.* 79.

1847. * STEWART, JAMES, M. D., Consulting Physician to the Northern Dispensary, N. Y. *Obit.* 12th September, 1864, *æt.* 65. *Trans.* 1.

1847. * STICKNEY, JOSIAH DWIGHT, M. D., *Obit.* 30th September, 1849, *æt.* 34.

1865. * STILES, R. CRESSON, M. D., Professor of Physiology in Berkshire Medical Institution, Mass.; Consulting Physician to Kings County Hospital, L. I. *Obit.* 17th April, 1873, *æt.* 42.

1876. STIMSON, D. M., M. D., Professor of Anatomy in the Woman's Medical College of the New York Infirmary.

Original. * STIMPSON, EDWIN B., M. D., late Physician to the New York Lying-in Asylum. *Obit.* 15th May, 1858, *æt.* 36.

1865. STIRLING, THOMAS B., M. D., Resident Physician New York Lying-in Asylum.

1876. ST. JOHN, SAMUEL B., M. D., Physician to New York Dispensary.

Original. * STONE, JOHN O., M. D., late Surgeon to Bellevue Hospital, N. Y. *Obit.* 7th June, 1876, *æt.* 63. *Bul.* 1.

Original. STORER, EBENEZER, M. D.

1874. STRACHAN, A. RUSSELL, M. D.

1859. SWAN, CHARLES Y., M. D.

Original. * SWEENEY, HUGH, M. D., *Obit.* 15th Sept., 1857, *æt.* 52.

ELECTED.

1851. * SWEENEY, JAMES, M. D., *Obit.* 1872.

1847. * SWETT, JOHN A., M. D., Professor of the Theory and Practice of Medicine in the University of the City of New York; Physician to the New York Hospital; Orator 1853. *Obit.* 18th September, 1854, *æt.* 45.

1866. * SWIFT, FOSTER, M. D., late Professor of Obstetrics and the Diseases of Women and Children in Long Island Hospital College, Brooklyn, L. I. *Obit.* 10th May, 1875, *æt.* 41.

1876. SWIFT, SAMUEL, M. D., Physician to St. John's Riverside Hospital, Yonkers, N. Y.

Original. * TAFT, MARCUS L., M. D., A. S. 1848. *Obit.* 8th February, 1850, *æt.* 29.

1872. TAUSZKY, RUDOLPH, M. D.

1867. TAYLOR, CHARLES F., M. D., Surgeon to the Orthopædic Dispensary, N. Y. *Bul.* 2.

Original. TAYLOR, ISAAC E., M. D., President of, and Emeritus Professor of Obstetrics and the Diseases of Women in Bellevue Hospital Medical College, N. Y.; Consulting Physician to Bellevue, Charity, and Infants' Hospitals; V. P. 1857 and 1858; Trust. 1872, now in office. *Trans.* 2; *Bul.* 3.

1862. TEATS, SYLVESTER, M. D.

1848. TELCAMPF, THEODORE A., M. D., late Physician-in-Chief to New York State Emigrants' Hospital.

1865. TELLER, SELIGMAN, M. D., late Physician to Mount Sinai Hospital, N. Y.

1847. * THAYER, HENRY W., M. D., *Obit.* 1857.

1859. THEBAUD, JULIUS S., M. D., Surgeon to St. Vincent's Hospital, N. Y.

1857. THOMAS, T. GAILLARD, M. D., Professor of Obstetrics and the Diseases of Women and Children in the College of Physicians and Surgeons, N. Y.; Physician to Bellevue Hospital, N. Y.; Surgeon to the New York State Woman's Hospital, N. Y.; R. S. 1858 to 1861. *Trans.* 2; *Bul.* 1.

1869. THOMPSON, GEORGE, M. D., Surgeon to the New York Dispensary.

1864. THOMS, WILLIAM F., M. D., Statistical Secretary 1868 to 1873. *Bul.* 1.

1864. THOMSON, WILLIAM H., M. D., Professor of Materia Medica and Therapeutics in the University of the City of New York; Physician to Bellevue Hospital, N. Y. *Bul.* 2.

ELECTED.
1867. *TOWNSEND, JOHN F., M. D., *Obit.* 8th January, 1874, *æt.* 64.

Original. *TOWNSEND, PETER S., M. D., *Obit.* 26th March, 1849, *æt.* 54.

1857. TUCKER, CHARLES P., M. D., Physician to the Home for Friendless Women, New York.

1863. *TUCKER, GEORGE H., M. D., *Obit.* 25th January, 1863, *æt.* 35.

1854. *TUTTLE, JOHN T., M. D., *Obit.* 27th January, 1870, *æt.* 68.

1854. *UHL, DAVID, M. D., *Obit.* 17th September, 1858, *æt.* 36.

Original. *UNDERHILL, ALFRED, M. D., V. P. 1863 to 1866; Trust. 1866 to 1873. *Obit.* 7th December, 1873, *æt.* 64. *Bul.* 2.

1847. *VACHE, ALEXANDER F., M. D., Physician to the Marine Hospital, S. I. *Obit.* 9th June, 1857, *æt.* 57.

Original. *VAN ARSDALE, HENRY, M. D., Morristown, N. J. *Obit.* 25th January, 1864.

Original. VAN ARSDALE, HENRY, M. D.

1847. *VAN ARSDALE, PETER, M. D., *Obit.* 1858.

1856. *VAN BEUREN, PETER, M. D., *Obit.* 5th December, 1873, *æt.* 71.

Original. *VAN BUREN, THOMAS, M. D., *Obit.* 1848.

Original. VAN BUREN, WILLIAM H., M. D., Professor of the Principles of Surgery and Diseases of the Genito-Urinary Organs and Clinical Surgery in Bellevue Hospital Medical College, N. Y.; Consulting Surgeon to New York and Bellevue Hospitals, N. Y.; V. P. 1858. *Trans.* 3; *Bul.* 1.

1859. VAN DOREN, MATTHEW D., M. D.

Original. *VAN KLEEK, JOHN R., M. D., late President of the New York County Medical Society, and of the Society for Relief of Widows and Orphans of Medical Men; Trust. 1861 to 1866. *Obit.* 2d January, 1876, *æt.* 66.

1847. VAN PELT, MOSES D., M. D., V. P. 1860 to 1863; Trust. 1864 to 1869.

1847. VAN WINKLE, EDWARD H., M. D.

1847. VANDERPOEL, EDWARD, M. D.

1859. *VANDERVEER, JACOB H., M. D., *Obit.* 20th August, 1873, *æt.* 55.

Original. VADERVOORT, JOHN L., M. D., R. S. 1849.

1847. VARICK, THEODORE R., M. D., Surgeon to Charity and St. Francis's Hospitals.

ELECTED.

1862. * VEDDER, Joseph H., M. D., *Obit.* 18th July, 1864, *æt.* 33.

1854. * VON ROTH, WOLDEMAN, M. D., *Obit.* 1857.

1876. WAGNER, CLINTON, M. D., Physician to Metropolitan Throat Hospital, N. Y.

1870. WALSER, THEODORE, M. D., late Deputy Health-Officer of the Port of New York, New Brighton, S. I. *Trans.* 1.

1873. WARD, EDWIN F., M. D., late Physician to the Demilt Dispensary, N. Y.

1863. WARNER, EVERARDUS B., M. D., House Surgeon to the Northern Dispensary, N. Y.

Original. * WASHINGTON, JAMES A., M. D., *Obit.* 30th August, 1847. *æt.* 45.

1853. * WATSON, JOHN, M. D., Surgeon to the New York Hospital; P. 1859 and 1860; Orator 1855 and 1860. *Obit.* 3d June, 1863. *Trans.* 1; *Bul.* 1.

Original. * WATTS, ROBERT, Jr., M. D., Professor of Anatomy in the College of Physicians and Surgeons, N. Y.; T. 1847. *Obit.* 8th September, 1867, *æt.* 55.

1876. WEBB, Z. SWIFT, M. D.

1867. WEBER, LEONARD, M. D.

Original. * WEEKS, CYRUS, M. D., *Obit.* 20th September, 1875, *æt.* 68.

1866. WEIR, ROBERT F., M. D., Surgeon to St. Luke's, Roosevelt, and the New York Hospitals; Surgeon to the New York Eye and Ear Infirmary.

1870. WEISSE, FANEUIL D., M. D., Professor of Practical and Surgical Anatomy in the University of the City of New York.

Original. WELLS, OVID P., M. D.

Original. * WHITE, AMBROSE L., M. D., late Physician to the Eastern Dispensary, N. Y. *Obit.* 2d June, 1865, *æt.* 61.

1858. WHITE, FRANCIS V., M. D., late Physician to the Eastern Dispensary, N. Y.

Original. WHITE, OLIVER, M. D., Consulting Physician to the Presbyterian Hospital, N. Y.; V. P. 1866 to 1870; Trust. 1871 to 1876.

Original. * WHITE, SAMUEL P., M. D., late Professor of Surgery in the Berkshire Medical Institution; Trust. 1853 to 1858. *Obit.* 6th June, 1867, *æt.* 65.

1875. WHITE, WHITMAN V., M. D.

1867. WHITE, WILLIAM T., M. D., Physician to Charity Hospital; Surgeon to the Presbyterian Hospital; Surgeon

ELECTED.

to the Demilt Dispensary, N. Y.; A. S. 1868 to 1870; R. S. 1871 to 1876, now in office.

1869. WHITEHEAD, WILLIAM R., M. D., late Physician to the Northwestern Dispensary, N. Y.

1875. WIENER, JOSEPH, M. D.

1847. WILKES, GEORGE, M. D., Consulting Surgeon to the New York Eye and Ear Infirmary.

Original. * WILLIAMS, MERRILL W., M. D., *Obit.* 3d December, 1873, *æt.* 72.

1860. * WINCHELL, MARTIN E., M. D., *Obit.* 1st May, 1864, *æt.* 35. *Bul.* 1.

1869. WINSTON, GUSTAVUS S., M. D., late Physician to the Demilt Dispensary, N. Y.

Original. * WOOD, ISAAC, M. D., Consulting Physician to Bellevue Hospital, N. Y.; P. 1850 and 1853; V. P. 1849; Trust. 1851 and 1852, 1859 to 1863. *Obit.* 25th March, 1868, *æt.* 74.

Original. WOOD, JAMES R., M. D., LL. D., Emeritus Professor of Surgery in Bellevue Hospital Medical College, N. Y.; Consulting Surgeon to Charity and St. Vincent's Hospitals, N. Y.; Surgeon to Bellevue Hospital, N. Y.; V. P. 1857.

Original. WOOD, STEPHEN, M. D.

1857. WOODHULL, HENRY W. B., M. D.

1852. * WOODWARD, GEORGE F., M. D., *Obit.* 1857.

1871. WOOLEY, JAMES V. S., M. D., Physician to the Presbyterian Hospital and to the Presbyterian Home for Aged Women, N. Y.

Original. WORSTER, JOSEPH, M. D.

1869. WRIGHT, CHARLES, M. D.

1875. WYLIE, W. GILL, M. D. *Trans.* 1.

1873. YALE, LE ROY M., M. D., Surgeon to Charity Hospital, N. Y.; Lecturer Adjunct upon Orthopedic Surgery in Bellevue Hospital Medical College, N. Y.

NON-RESIDENT FELLOWS.

---

* Berger, Francis E., M. D., Paris, France. *Obit.* 1st February, 1866, *æt.* 77.

Corson, John W., M. D., late Physician to the New York Dispensary, Orange, N. J.

Draper, John W., M. D., LL. D.

Dunster, Edward S., M. D., late Professor of Midwifery and Diseases of Women in Vermont Medical College.

Hepburn, James C., M. D., Japan.

Johnston, Frank U., Jr., M. D., Cooperstown, N. Y.

* Lee, Charles Alfred, M. D., Emeritus Professor of Materia Medica and Hygiene in Buffalo Medical College, Peekskill, N. Y. *Obit.* 14th February, 1872, *æt.* 71.

* Lewis, William B., M. D., Florida. *Obit.* 16th June, 1874, *æt.* 32.

Merritt, J. King, M. D., Flushing, L. I.

North, Nelson J., M. D., South Carolina.

Sands, Austin L., M. D., Newport, R. I.

Shrady, George F., M. D., New York City.

Sloan, William J., M. D., U. S. A.

Stewart, F. Campbell, M. D.

Thompson, Bradford S., M. D., Salisbury, Conn.

* Van Arsdale, Henry, M. D., Morristown, N. J. *Obit.* 25th January, 1864.

Vermilye, William E., M. D., Pittsfield, Mass.

HONORARY FELLOWS.

Elected.	
1871.	Bowditch, Henry I., M. D., Professor of Clinical Medicine in Harvard University, Boston, Mass.
1859.	*Gibson, William, M. D., Professor of the Principles and Practice of Surgery in the University of Pennsylvania, Philadelphia, Pa. *Obit.* 2d March, 1868, *æt.* 80.
1876.	Gross, Samuel D., M. D., LL. D., Professor of Surgery in Jefferson Medical College, Philadelphia, Pa.
1857.	*Ives, Eli, M. D., Professor of Materia Medica and Botany in Yale College, New Haven, Conn. *Obit.* 8th October, 1861, *æt.* 82.
1860.	*Jackson, James, M. D., Professor Emeritus of the Practice of Physic in Harvard University, Cambridge, Mass. *Obit.* 27th August, 1867, *æt.* 90.
1859.	*La Roche, Rena, M. D., of Philadelphia, Pa. *Obit.* 9th December, 1872, *æt.* 77.
1859.	*Mussey, Reuben D., M. D., Professor of Surgery in the Medical College of Ohio, Cincinnati, O. *Obit.* 21st June, 1866, *æt.* 86.
1857.	*Spaulding, Mathias, M. D., of Amherst, Mass. *Obit.* 22d May, 1865, *æt.* 95.
1871.	Stillé, Alfred, M. D., Professor of Clinical Medicine in the University of Pennsylvania, Philadelphia, Pa.
1874.	Vanderpoel, S. Oakley, M. D., Professor of Theory and Practice and Clinical Medicine in Albany Medical College, Albany, N. Y.; Health Officer of the Port of New York.
1871.	Wood, George B., M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania, Philadelphia, Pa.

CORRESPONDING FELLOWS.

Elected.	
1873.	Acland, Henry W., M. D., F. R. S., Regius Professor of Medicine in the University of Oxford, England.
1856.	* Adams, Francis, M. D., LL. D., Surgeon, Banchory, Scotland.
1847.	* Agassiz, Louis John R., M. D., F. R. S., Professor of Natural History in Harvard University, Cambridge, Mass. *Obit.* 14th December, 1873, *æt.* 66.
1850.	* Amussat, Jean Zuilme, M. D., Member of the Royal Academy of Medicine, Paris, France. *Obit.* 12th May, 1856, *æt.* 58.
1854.	* Andral, Gabriel, M. D., Professor of Pathology in the Faculty of Medicine, Paris, France. *Obit.* 13th February, 1876, *æt.* 78.
1854.	Barthez, Ernst, M. D., Paris, France.
1847.	* Beck, Theodrick Romeyn, M. D., Professor of Medical Jurisprudence in Albany Medical College. *Obit.* 19th November, 1855, *æt.* 64.
1857.	* Bennett, John Hughes, M. D., Professor of Medicine in the University of Edinburgh, Scotland. *Obit.* 25th September, 1875, *æt.* 63.
1854.	Bigelow, Jacob, M. D., Professor of Materia Medica in Harvard University, Cambridge, Mass.
1847.	* Blatchford, Thomas W., M. D., Troy, N. Y. *Obit.* 7th January, 1866, *æt.* 71.
1867.	Brown-Séquard, C. E., M. D., Paris, France.
1849.	* Bureaud-Riofrey, A. M., M. D.
1871.	Chambers, Thomas K., M. D., Physician to and Lecturer on Medicine at St. Mary's Hospital, London.
1847.	* Civiale, Jean, M. D., Honorary Member of the Academy of Medicine, Paris, France. *Obit.* 13th June, 1867, *æt.* 75.

ELECTED.

1866. CUNHA, JOSÉ DE, M. D., Rio Janeiro, Brazil.

1868. DAVIS, NATHAN S., M. D., Professor of the Principles and Practice of Medicine in Chicago Medical College.

1872. DICHIARA, FRANCESCO, M. D., Palermo, Italy.

1850. * DICKSON, SAMUEL H., M. D., Professor of the Institutes of Medicine in South Carolina Medical College. *Obit.* 31st March, 1872, *æt.* 74.

1871. DICKINSON, WILLIAM H., M. D., Physician to and Lecturer on Pathology at St. George's Hospital, London, England.

1854. * DUBOIS, Baron PAUL, Dean and Professor of Clinical Midwifery in the Faculty of Medicine of Paris, France. *Obit.* 29th November, 1871, *æt.* 76.

1867. DUMONT, HENRI, M. D., Havana, Cuba.

1848. DUPIERRIS, MARTIAL, M. D., Havana, Cuba.

1876. ERICKSEN, JOHN ERIC, F. R. C. S., London, England.

1850. * FENNER, ERASMUS D., M. D., Professor of the Theory and Practice of Medicine in New Orleans School of Medicine. *Obit.* 4th May, 1866, *æt.* 59.

1849. FERGUSON, Sir WILLIAM, F. R. S., Surgeon to King's College Hospital, London, England.

1851. GROSS, SAMUEL D., M. D., late Professor of Surgery in the Medical Department of Louisville University, Louisville, Ky.

1854. * GUGGENBUHL, J., M. D., Paris, France.

1847. * HARRIS, THOMAS, M. D., Surgeon-General U. S. Navy, Washington, D. C. *Obit.* 4th March, 1862.

1848. * HOLLAND, Sir HENRY, Bart., M. D., D. C. L., LL. D., F. R. S., Physician to H. M. the Queen, London, England. *Obit.* 29th October, 1873, *æt.* 85.

1850. * HOOKER, WORTHINGTON, M. D., Professor of the Theory and Practice of Medicine in Yale College, New Haven, Conn. *Obit.* 6th November, 1867, *æt.* 61.

1874. JENNER, Sir WILLIAM, Bart., M. D., D. C. L., F. R. S., Professor of Clinical Medicine in University College, London, England.

1853. * LEROY DE ETIOLLES, JEAN J. J., M. D., Paris, France, *Obit.* August, 1860, *æt.* 62.

1871. * NÉLATON, AUGUSTE, Professor of Clinical Surgery in the University of Paris, France. *Obit.* 20th September, 1873, *æt.* 65.

1874. MITCHELL, S. WEIR, M. D., Philadelphia.

ELECTED.

1874. OWEN, RICHARD, M. D., Hunterian Professor in the Royal College of Surgeons, London, England.

1874. PAGET, Sir JAMES, Bart., M. D., F. R. S., D. C. L., Consulting Surgeon to St. Bartholomew's Hospital, London, England.

1857. PEASLEE, EDMUND R., M. D., Professor of Surgery in Dartmouth Medical College, Hanover, N. H.

1853. PRINCE VIROMMA LUANG SI TIRAT SANIK, Siam.

1868. POST, GEORGE E., M. D., Beirut, Syria.

1874. REGO, FILHO JOSÉ PEREIRA, M. D., Rio Janeiro, Brazil.

1850. REYNOLDS, EDWARD, M. D., Boston, Mass.

1852. RICORD, PHILIPPE, M. D., Member of the Royal Academy of Medicine, Paris, France.

1854. RILLIET, FRANÇOIS, M. D., Paris, France.

1860. ROESSER, D. P., M. D., Athens, Greece.

1849. * ROGET, PETER MARK, M. D., F. R. S., Professor of Physiology in the Royal College, London, England. *Obit.* 17th September, 1869, *æt.* 90.

1871. ROKITANSKY, CARL, M. D., Professor of Pathology in the University of Vienna.

1856. * SIMPSON, JAMES Y., M. D., Professor of Midwifery in the University of Edinburgh, Scotland. *Obit.* 8th May, 1870, *æt.* 59.

1848. SMITH, ASHBEL, M. D., Texas.

1874. STEWART, F. CAMPBELL, M. D.

1872. TILT, EDWARD J., M. D., Consulting Physician to the Farringdon General Dispensary, London, England.

1852. TOWNSEND, JAMES C., M. D., Long Island, N. Y.

1852. * VELPEAU, ALFRED A. L. M., Professor of Clinical Surgery in the Faculty of Medicine of Paris, Surgeon to the Hôpital de la Charité, Paris. *Obit.* 23d August, 1867, *æt.* 72.

1871. VIRCHOW, RUDOLPH, M. D., Professor of Pathological Anatomy in the University of Berlin.

1869. VOSS, LOTHAR H., M. D., of Berleberg, Prussia.

1874. WHITE, JAMES P., M. D., Buffalo, N. Y.

1857. * WING, JOEL A., M. D., late President New York State Medical Society, Albany, N. Y. *Obit.* 6th September, 1852, *æt.* 64.

1867. WORTABET, JOHN, M. D., Beirut, Syria.

LIST OF CONTRIBUTORS

TO THE

BUILDING FUND OF THE NEW YORK ACADEMY OF MEDICINE,

WITH THE AMOUNTS PAID TO JULY, 1876.

Published by Order of the Council.

John G. Adams, M. D.	$500 00
Cornelius R. Agnew, M. D.	50 00
James Anderson, M. D.	500 00
James H. Anderson, M. D.	50 00
Thomas S. Bahan, M. D.	25 00
James L. Banks, M. D.	300 00
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* Deceased.

John Burke, M. D.	$100 00
Frederick A. Burrall, M. D.	50 00
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TRANSACTIONS

OF THE

NEW YORK ACADEMY OF MEDICINE.

ABSCESSES ORIGINATING IN THE RIGHT ILIAC FOSSA, WITH STATISTICS.

By GURDON BUCK, M.D.,

VISITING SURGEON TO NEW YORK AND PRESBYTERIAN HOSPITALS; CONSULTING SURGEON TO ROOSEVELT AND ST. LUKE'S HOSPITALS.

Read September 17, 1874.

In preparing this paper on abscesses in the right iliac fossa, my object has been not so much to prepare a complete treatise on the subject as to present the most practical points involved in the diagnosis and treatment of this formidable disease, as illustrated by cases that have occurred in my own practice or that of my colleagues.

Abscesses in the lower portion of the abdominal cavity and its parietes may be divided into three classes:

The first class comprises abscesses produced by perityphlitis, or, as the term implies, inflammation around the cæcum, due most frequently to perforation of the vermiform appendix. The collection of pus formed in this abscess occupies the peritoneal cavity itself, and is walled in by adhesions.

The second class comprises abscesses originating in the iliac fossa of either side, and situated underneath and inclosed by the iliac fascia.

The third class comprises abscesses developed in the con-

nective tissue external to the peritonæum, and between it and the parietes of the abdominal cavity. Besides these distinct classes of abscesses, collections of pus may form in the same localities as the abscesses of the second and third classes; but, in these cases, the pus will be found to have migrated from its original place of origin at a distance, the lumbar region being the most frequent seat of origin of such collections.

It is my object, however, only to treat of the first class of abscesses; those resulting from perityphlitis, and occupying the right iliac fossa.

The following case occurred recently in my own practice:

Case I.—G. N., a lad, aged twelve years, residing in this city, while in the enjoyment of ordinary good health, and after a day of accustomed activity, was, on Thursday, June 11th, attacked, on going to bed, with violent colic-pains and vomiting. Hot poultices and anodynes were resorted to, and on the following day free evacuations from the bowels were obtained by a dose of five grains each of calomel and Dover's powder. On Saturday evening, June 13th, forty-eight hours after the attack, I saw him in consultation with Dr. J. Linsly, the attending physician, and found his condition as follows: Vomiting had almost ceased, his pulse was moderately accelerated, and the temperature of the surface was normal. The abdomen was tumid, but not tense; and tenderness on pressure, which at first had been diffused over the whole abdomen, was now confined to the right iliac region, where a deep-seated tumor could be defined, though rather indistinctly. It occupied a space above the outer half of Poupart's ligament, and close to it. There was no elevation of the surface over the tumor, and the pain produced by the most cautious attempts at deep pressure, deterred from as thorough an exploration as might have been desirable. No dullness on percussion was appreciable, and flexion and extension of the thigh caused no pain. The tongue was clean, and there was no aversion to taking nourishment. Dr. Linsly had already prescribed pills of s. morph. gr. $\frac{1}{6}$, which were to be continued every two hours. The conclusion we arrived at respecting our patient was that an abscess

was forming in connection with perforation of the vermiform appendix. Six leeches were applied over the tumor the same evening, and followed by poultices. Under the influence of the morphia pills a quiet condition was maintained; the pulse ranged from 80 to 100; the temperature of the surface was uniformly natural. On Wednesday (seventh day) the morphia was suspended, and a movement of the bowels obtained by means of ol. ricin. ℥ss, and an enema of catnep-tea, but with only partial effect, owing to his instinctively avoiding any straining effort. After the suspension of the morphia he became nervous, and complained more of pain in the tumor, especially after an examination of it, however carefully made. No chill or feverishness was at any time observable, nor any extension of abdominal tenderness beyond the limits of the tumor itself. Nothing abnormal could be felt by digital exploration *per rectum.* On Thursday, 18th (eighth day), Prof. W. Parker joined our consultation, and arrived unhesitatingly at the conclusion that an abscess had formed, and required to be opened from the surface. The absence of dullness on percussion over the tumor was the only condition that did not concur in establishing the diagnosis arrived at. All the other conditions and antecedents corroborated it; and the existence of resonance was afterward explained by the presence, within the cavity of the abscess, of an abundant collection of gas. The morphia and poultices were resumed, preparatory to an operation on the following day. On Friday (ninth day), at twelve o'clock, the operation was performed, after the inhalation of ether. Some elevation of the surface over the tumor was now manifest, but no redness, or œdematous infiltration, or adhesion of the skin to the underlying parts, existed; nor was any fluctuation perceptible. By a bolder palpation, now admissible under anæsthesia, the deeper outlines of the tumor could be better defined, and its longest diameter was ascertained to be parallel with Poupart's ligament. The operation was performed as follows: A point two fingers' breadth distant from and to the inside of the anterior superior spinous process of the ilium, and a little below its level, where the tumor ap-

proached nearest to the surface, was chosen for an opening, which was made with a small canulated trocar (equivalent in size to No. 1 bougie scale). A puncture was first made at the point chosen with a tenotomy-knife, through the skin, to facilitate the onward passage of the trocar. This was then inserted, and advanced till it encountered the tendon of the ext. oblique muscle, which presented great resistance to its further passage. To overcome the resistance safely, the trocar was withdrawn within its sheath, and the canula held in firm contact with the surface of the tendon, while the point of the trocar was pushed on. By successive repetitions of this manœuvre, the trocar at length encountered no further resistance; and, on being withdrawn entirely while the canula was advanced, matter escaped from its outer orifice, and the success of the procedure was demonstrated. The canula, being still held *in situ*, served as a guide, along the outer surface of which a sharp-pointed knife was conducted into the cavity of the abscess, and used to enlarge the track of the canula. On withdrawing the knife, the wound was enlarged to the extent of more than an inch at the surface of the skin. The little-finger was then thrust into the cavity of the abscess, and the opening dilated sufficiently to allow a free escape of the matter, which was fetid and of a dirty-grayish aspect, but without any biliary discoloration. With the matter there was also an abundant escape of fetid gas, to the presence of which may be attributed the resonance on percussion over the tumor. A plug of cotton-wick well greased was inserted in the opening, and the poultices resumed. For the first three or four days an injection of salt-and-water (ʒj to ℥viij) was thrown into the abscess at the daily dressing. His subsequent progress was favorable. On the fifth day after the operation, when the discharge was no longer fetid, and had regained a healthy character, two tufts of sloughy connective tissue came away, followed by a more copious discharge of pus, which had accumulated in the cavity of the abscess from obstruction of the outlet. On June 25th, the sixth day after the operation, a dark-colored, gritty substance, of the size of a small pea, was discharged. A

chemical analysis ascertained it to be a phosphatic concretion. On the day following, another substance, of the shape and size of half an inch of small clay pipe-stem, was found on the dressing; it had all the characters of compact fecal matter. After this, the suppuration progressively diminished. On June 29th a third smaller tuft of sloughy tissue was discharged. His subsequent progress requires no special notice. Under excellent care at home, with generous diet and a moderate allowance of stimulants and tonics, he steadily improved both locally and generally. On July 18th he accompanied his family to their summer residence in the country, where he continued to gain rapidly. On August 14th his father reported that the wound had healed, and he was quite himself again.

In the *Medical Record* of March 15, 1867, Prof. Willard Parker reported a case of this disease in which he first employed successfully a method of treatment which may be said to have disarmed this disease of its terrors, and changed its issue from an almost invariably fatal result to the reverse. This method consists in making an early incision into the abscess without waiting for fluctuation to demonstrate the presence of pus. In the *Medical Record* of 15th of June following, Dr. J. H. Hobart Burge, of Brooklyn, reported a second successful operation performed by Dr. Parker on a patient of Dr. Burge's. Since the publication of these two cases, two others equally successful have been reported in the *New York Medical Journal*, one by Dr. Leonard Weber, in the August number of 1871, the other by Prof. H. B. Sands, in the August number of 1874. Other cases, not yet made public, have been ascertained to have occurred in the practice of other surgeons, to whose courtesy the author is indebted for such particulars as will enable him to develop more fully this interesting subject. These cases, together with one reported by Mr. Hancock, of London, in 1848, which will be more particularly noticed hereafter, and the one in my own practice just narrated, form an aggregate of ten cases, from which the following deductions may be drawn:

TABLE OF TEN CASES OF ILEO-CÆCAL ABSCESS FOLLOWING PERFORATION OF VERMIFORM APPENDIX.

No.	Name of Operator, where recorded.	Date of Operation.	Sex.	Age.	Invasion and Localization of Disease.	Day of Operation.	Mode of Operation.	Nature of Discharge	Foreign substances discharged on what day.	Result.	REMARKS.
1	Hancock, London Med. Gazette, New Series, vol. viii., p. 547.	1848	F	Adult.	Abrupt, with symptoms of acute peritonitis early localized in ileo-cæcal region.	9	Incision over tumor 4″ long.	Fetid, with gas.	Two fecal lumps, incrusted with concretion 15 day.	Recovery.	Attacked the day after confinement with fifth child. No fluctuation felt.
2	Prof. W. Parker, M. D., N. Y. Med. Record, March 15th.	1867	M	40	Do. do. do. do.	9	Incision 6″ long, down to fascia tranversalis, then exploring-needle inserted.	Fetid.	None.	Recovery.	No fluctuation felt.
3	W. Parker, M. D. Reported by Dr. J. H. H. Burge, N. Y. Med. Rec., June 15th.	1867	F	15	Do. do. do. do.	14	Incision 3′ long.	Fetid.	None.	Recovery.	No fluctuation felt.
4	L. Weber, M. D., N. Y. Med. Journal, August.	1871	M	22	Do. do. do. do.	7	Incision 9″ long, down to fascia, then left to open spontaneously.	Fetid.	Concretion on 16th day.	Recovery.	No fluctuation felt.—Spontaneous opening, and discharge took place two and a half days after operation.
5	E. Krackowizer, M.D. Communicated.	1872	M	.2	Do. do. do. do.	11	Incision down to fascia transv., then divided on director.	Fetid, no stain of bile.	None seen.	Recovery.	No fluctuation felt.
6	Sam. B. Ward, M. D. Communicated.	1872	M	17	Do. do. do. do.	early.	Incision as for ligature of ext. iliac art. down to fasc. trans. Left to open spontaneously.	Fetid, no stain of bile.	None seen.	Recovery.	No fluctuation felt.—Spontaneous opening on second day after operation.
7	Prof. H. B. Sands, M. D., N. Y. Med. Journal, August.	1874	M	41	Do. do. do. do.	13	Incision down to fascia, then trocar.	Fetid.	8 or 9 concretions.	Recovery.	No fluctuation felt.
8	C. Kelsey, M. D. Communicated.	1874	F	16	Do. do. do. do.	8	Incision down to fasc., aspirated, then enlarged.	Fetid.	None seen.	Recovery.	No fluctuation felt.
9	J. P. P. White, M. D. Communicated.	1874	M	32	Do. do. do. do. Within two years previous, had two acute attacks, relieved without discharge, but leaving a deep-seated tumor in ileo-cæcal region.	9, 11 } Aspirated. 15	Incision 5′ long.	Fetid, no stain of bile.	Concretion like a date-pit.	Recovery.	Fluctuation distinct.—First aspiration on ninth day. Drew off 26 ounces pus. Second aspiration on 11th day. Drew off sixteen ounces. Free opening made on 15th day.
10	Gurdon Buck, M. D. Narrated herein.	1874	M	12	Do. do. do. do.	9	Punctured first with fine trocar, then enlarged by incision with knife.	Fetid, no stain of bile.	1 concret'n, 1 lump fæces, 3 tufts of slough.	Recovery.	No fluctuation felt.

Seven were males and three females. Their ages were as follows: two males were twelve; one female fifteen; one sixteen; one male seventeen; four males were respectively twenty-two, thirty-two, forty, and forty-one; one female was the mother of five children. In every case the invasion of the disease was abrupt, and attended with symptoms of acute peritonitis, which early became localized in the ilio-cæcal region, and in all a deep-seated tumor was more or less distinctly felt at an early period in the same locality. In nine of the ten cases no fluctuation could be detected at the time of the operation, which was performed on the ninth day after the attack in four cases, in one case early, so stated, and in one case each on the seventh, eighth, eleventh, thirteenth, and fourteenth days respectively. A discharge of fetid matter took place after the opening of the tumor in all cases, and in some it was accompanied with gas-bubbles. In four cases it was stated that the matter was not stained with bile, from which it might be inferred that there existed no open communication between the intestine and the cavity of the abscess; the same was probably true of the other six cases in which the fact of biliary discoloration was not stated. In five of the cases one or more foreign substances were discharged at varying intervals after the operation, and in the other five cases none were found. Where none was found it may be presumed that, if retained in the cavity of the abscess, it became embedded in exudation material, and thus was rendered innocuous. These substances were concretions, and fecal masses incrusted with concretion. In one case three tufts of sloughy connective tissue were discharged besides the foreign substances. In one case (No. 9), communicated by Dr. White, the collection of pus was excessive in quantity, and was therefore readily detected by existing fluctuation. The patient became collapsed on the ninth day, and was in imminent danger of his life. Instead of a free opening being made for the exit of matter, the aspirator was used, and twenty-six ounces of pus drawn off; two days after, sixteen ounces were drawn off by a second aspiration, and four days after the second aspiration a free opening was established.

The method of operating inaugurated by Dr. Parker, and adopted by his imitators, was the following: An incision three to six inches in length was carried across the tumor a little above and nearly parallel with Poupart's ligament, and divided the skin and subjacent tendinous and muscular layers till the fascia transversalis was exposed. An exploring-needle, or fine trocar, was then inserted in search of matter, and the puncture afterward enlarged to a free opening with a knife. In one case (No. 8), after the exposure of the fascia, the aspirator was first used, and then a free opening made. In two other cases (Nos. 4 and 6), after the exposure of the fascia transversalis, fluctuation not being perceptible, the wound was dressed open, and a spontaneous opening formed on the second and third days after.

This cautious procedure was adopted, no doubt, as a surer means of avoiding a wound of the intestines. In my own case, however, I deviated from this plan by first penetrating the abscess without any preliminary incision of the abdominal parietes, and then enlarging the punctured track sufficiently to afford a free outlet for the contents of the abscess. The reasons for this modification of the operation were these: Regarding the most prominent point of the tumor as an indication of the approach to the surface of pus, and not of intestine, this point was chosen for the insertion of a fine, canulated trocar, or, what is preferable, a sharp-pointed canula, such as is used in the operation of aspiration. The insertion of such a small-sized canula into the intestine itself would be harmless, inasmuch as on its withdrawal there would be no escape of fecal fluid, and consequently no danger of the formation of a fistula. Matter having been reached by this first step of the procedure, the canula is held *in situ*, and used as a guide, along the outer surface of which a sharp-pointed knife is conducted into the cavity of the abscess, and the track of the canula enlarged to the requisite dimensions. In case of a failure to reach the collection of pus by a first attempt, a second introduction of the canula may be safely tried at another selected point. By this method, also, an extensive incision

of the abdominal parietes is avoided, and the subsequent liability to a hernial protrusion prevented. After completing the opening with the knife, a finger should be introduced to stretch it, and a full-sized tent, well greased, should be kept in for the first three or four days after the operation.

Remarks. *Diagnosis.*—The abruptness of the onset of the disease, with symptoms of acute peritonitis, early becoming localized in the ileo-cæcal region, or restricted mostly to this region from the first of the attack, will distinguish it from the other forms of abscess that originate in this region, and from fecal accumulations in the cæcum, which are gradual in their development. Its precise locality in the hollow of the iliac fossa should also distinguish it from strangulated hernia, the attendant symptoms of which are not unlike those of perityphlitis. A close observation of the daily progress of the disease will also very much aid our judgment in arriving at a correct conclusion in regard to its nature.

Prognosis.—Authors who have treated of this disease have regarded it as almost invariably fatal in its termination. It is true, however, that sometimes recovery has taken place after the spontaneous formation of an opening into the intestine, probably the cæcum, and the discharge of the contents of the abscess *per anum.* A like favorable result has followed after an opening into the bladder, and the expulsion of pus *per urethram.* In much rarer instances the abscess has emptied itself by a spontaneous opening through the abdominal parietes. A favorable termination by resolution is perhaps of the rarest occurrence, and yet it has taken place. Happily, this disastrous tendency need now no longer exist, but may be averted by a seasonable operation.

Treatment.—In the onset and early progress of the disease, Mr. John Burne (in vol. xx. of "*Medico-Chirurgical Transactions*") very judiciously cautions against the energetic depleting treatment that might be applicable to acute idiopathic peritonitis. It is well to apply leeches early over the tumor to the extent of six to twelve in number, and to repeat them if necessary. Poultices are also indicated, but they

must be adjusted so as not to be burdensome by their weight. Five to ten grains of calomel, followed by ol. ricini, with the addition of tinct. opii, or sol. s. morph., should be given. After this a state of moderate narcotism should be maintained by the administration of pil. opii gr. j or sulph. morphia $\frac{1}{6}$ to $\frac{1}{4}$ gr., repeated at first every hour till their effect is produced, and afterward at intervals of two to four hours. The object of our treatment should be to moderate the production of pus, and thereby diminish the strain upon the adhesions which wall in the abscess, and shut it off from the general peritoneal cavity, till the favorable moment arrives for giving exit to the matter through an external opening upon the surface of the abdomen. To determine the time of operating is a point of chief importance. It should be borne in mind that we are not to wait to detect fluctuation, which is regarded as the unequivocal sign of the existence of matter. Before that point is reached, the patient is exposed to a disastrous issue from different sources, such as the giving way of the adhesions that wall in the abscess, and the supervention of fatal general peritonitis; from gangrene; and exhaustion from the hectic of purulent cachexia. If we interrogate experience on this point, we find that, in the ten cases cited in this paper, the operation was performed at the earliest moment on the seventh day after the onset of the disease, and at the latest on the fourteenth day. We may therefore, perhaps, safely lay it down as a rule that after the lapse of one week from the onset of the disease there should be no delay in resorting to the operation, unless there should be clear indications of resolution going on, which is an extremely rare issue of this disease. This treatment, so remarkably successful in the cases designated, is quite inapplicable to those other cases that prove rapidly fatal from general peritonitis. It should also be stated in this connection that Mr. Hancock, of London, performed this same operation with success in 1848, but, for some reason or other, his report of the case failed to receive the attention it deserved. In his report (*see London Medical Gazette*, New Series, vol. vii., p. 547) before the London Medical So-

ciety, of which he was then president, Mr. Hancock remarked as follows: "Abscesses of the abdomen connected with the cæcum or large intestines, and attended with fluctuation, had from time to time been opened, but he was not acquainted with any instance in which an operation had been attempted under the circumstances about to be detailed in his own case, and where the result had been so entirely satisfactory. In the cases recorded, the presence of fluctuation has proved the existence of matter, but the details of his case would show that we should not always wait for this unequivocal sign." His case was that of an adult female, in whom the attack began on the day following her giving birth to her fifth child, six or seven weeks before the full time, with a severe pain in the right groin and a sensation of something having snapped asunder as she turned herself over in bed. After nine days of appropriate treatment, Mr. Hancock operated by an incision four inches long carried across the tumor from the spine of the ilium inward above and close to Poupart's ligament. A quantity of fetid matter with gas-bubbles was discharged. On the fifteenth day after the operation, two masses of fæces, incrusted with calcareous deposit, and moulded upon each other, were discharged. From their size, Mr. Hancock judged that they had been impacted in, and had escaped by ulceration from the vermiform appendix. At a meeting of the same Society held March 27, 1871 (*British Medical Journal*, 1871, vol. i., p. 450), the subject of perityphlitis was brought forward, and, in the discussion that followed, no allusion was made to Mr. Hancock's method of treatment. Although Mr. Hancock's report was also republished in full in the *American Journal of Medical Sciences* of 1849, the only notice of it in this country was by Dr. George Lewis, then Physician to the Eastern Dispensary, in an article on "Abscesses in the Appendix Vermiformis," that appeared in the *New York Journal of Medicine*, 1856, Third Series, vol. i. Under the head of "Treatment" he remarks upon the question of the propriety of making a free incision downward upon the tumor, and states that he is inclined to favor the operation.

"The favorable issue of a single case, and this, so far as our information extends, the only one on record in which this practice was adopted, taken in conjunction with other considerations" (already stated by the writer), "if they do not conclusively settle the utility of this mode of procedure, at least justify a more extended trial of it." He then reproduces the report of Mr. Hancock's case in full. This important subject attracted no further notice, nor is any allusion to the operation to be found in the most recent text-books on medicine and surgery, such as Aitken, Reynolds, and Flint, or Holmes, Gross, or Hamilton. Happily for suffering humanity, this same method of treatment was reproduced by one of our own number, in 1867, as before stated, and has already had so many successful imitators that its vitality may now be considered as assured.

Besides abscesses following perforation of the vermiform appendix, there are lesions of the cæcum giving rise to abscess. Mr. John Burne ("*Medico-Chirurgical Transactions*" 1839, vol. xxii., p. 41) remarks: "Of perforative ulceration of the cæcum from within, no case verified by dissection has occurred under my own observation. One is described by Ferrall, in the *Edinburgh Medical and Surgical Journal*, vol. xxxvi., p. 12, Case No. 4, of tubercular ulceration, in which a tumor formed in the right groin, burst in a few days, and discharged fæces and caraway-seeds. On dissection, several ulcers were found in the cæcum, one of which had perforated its posterior wall and communicated directly with an abscess in the iliac fossa, the abscess also communicating with the external opening in the groin." Perforative ulceration of the cæcum from without, however, does occur in those cases in which abscesses following perforation of the appendix burst into the intestine, and are discharged *per anum*. A very remarkable case in which the cæcum was involved came to my knowledge in a recent visit to St. John, New Brunswick. It was communicated to me by Dr. William Bayard, an eminent practitioner of that city, to whose courtesy I am indebted for the following history of the case, drawn up by Dr. Thomas Walker, the medical attendant upon the patient:

Case II. *Worms discharged from the Cœcum through an Opening in the Abdominal Parietes.*—Was called suddenly on the morning of April 29, 1874, at six o'clock, to see Mrs. L., aged sixty-five. She had been in tolerably good health until four days ago, when she was seized with vomiting, rejecting every thing swallowed. She had suffered during the preceding twelve months with occasional attacks of pain across the bowels, which she had attributed to flatulence, and which had generally been promptly relieved by the use of some warm carminative. During the present attack she did not appear to have suffered much pain. She was pale, partially insensible, with a small, feeble pulse, 96; tongue coated; bowels constipated. There had been no stercoraceous vomiting. On examining the abdomen, an oval swelling was found above Poupart's ligament, about four inches in length, extending from just above the crest of the ilium toward the symphysis pubis. It was hard, reddish, and had all the appearance of a pointing abscess. It had no impulse on coughing. She had not complained of pain there to any marked degree, though it was quite tender to the touch. Five grains of calomel were given, and a sedative and antispasmodic mixture ordered to be taken every three hours. She was allowed ice to suck, a sinapism was directed to be applied over the stomach, and a poultice to the swelling; also, beef-tea and brandy were ordered, in small quantities. At eleven o'clock in the forenoon the bowels had been freely moved; she had vomited only once since six o'clock, and had begun to retain every thing she took; pulse much stronger; she is quite sensible. Her condition remained much the same until May 1st, when the abscess having clearly pointed, and fluctuation being very distinct, it was opened about its middle, and a wineglassful of very fetid, dirty-brown pus escaped. Poultice to be continued, with the addition of a carbolic-acid lotion.

3*d.*—Abscess has discharged freely since it was opened. The discharge continues offensive; the edges of the opening have sloughed slightly, and another opening has formed, by ulceration, below the first. Bowels have been opened fre-

quently within twenty-four hours, the stools being thin and yellowish, but devoid of blood or pus.

4th.—On dressing the wound to-day I extracted from it a perfect worm (*Ascarus lumbricoides*), about six inches in length. Two similar worms, I was informed, had been found that morning on the poultice, with the discharge. Diarrhœa still continues.

5th.—Two more worms have been discharged from the opening, making five in all. Bowels still loose; the edges of the opening have sloughed still more.

7th.—She was seen by Dr. William Bayard; her condition has undergone no material change since the last report.

12th.—Diarrhœa has ceased; the edges of the opening have healed considerably, and now look healthy; the discharge has very much diminished in quantity, and lost its offensive character.

13th.—Fæces have begun to come through the opening, and from this date forward they continued to pass, and the case presented all the characters of a case of artificial anus until she died, in August. The worms were all dead when discharged, and one of them came away in two pieces.

NOTE.—Since the foregoing article was read at a stated meeting of the New York Academy of Medicine, held September 17, 1874, the author has received a communication from Prof. James R. Wood, M. D., of which the following extract relates to this subject:

"I have operated on three cases by Dr. Parker's method; the patients were all males and adults. In all of them the onset of the disease was abrupt, with acute symptoms of peritonitis that became localized in the right iliac region. In two of the cases the offending substance was discharged; in the third case none was seen. Two cases recovered rapidly; one died on the second day after the operation. Although I saw these cases early after the attack, I did not operate earlier than the seventh day. I think it is as important not to operate too soon as it is to defer the operation too long."

CASE OF ABSCESS ORIGINATING IN RIGHT ILIAC FOSSA, TREATED BY INCISION.

By ERNST KRACKOWIZER, M. D.,
SURGEON TO NEW YORK AND BELLEVUE HOSPITALS.

Read September 17, 1874.

G. Lauter, 60 Rivington Street, a fine boy, twelve years of age, was taken with pain in the abdomen early in the morning, October 6, 1872. No cause could be assigned. Notwithstanding, he went to school, whence, the pain increasing and vomiting supervening, he was sent home by the teacher.

Dr. C. Mohn saw him soon and found that the pain was seated in the ileo-cæcal region, aggravated on pressure. From there the pain spread over the whole abdominal cavity; in fact, the symptoms of general peritonitis were well marked on the second or third day after the seizure. No amelioration was produced by general and local antiphlogistic treatment.

October 17th—eleven days after the commencement of the disease—I was called in consultation. Besides the symptoms of general peritonitis, I detected a swelling, commencing $1\frac{1}{2}''$ interiorly from the *right* spinā superior anterior, and, following Poupart's ligament, extending beyond the median line as far as the external border of the *left* rectus abdominis muscle. From Poupart's ligament the swelling reached to a line equidistant from the navel and the symphysis pubis. The swelling was moderately resistant; no fluctuation could be felt.

An incision was made $\frac{3}{4}''$ above Poupart's ligament, commencing on the *outer* border of *right* rectus abdominis muscle, and carried outward toward the superior anterior spine of the ilium, ending $1''$ inside of this landmark. Layer after layer the aponeurosis of the external oblique, then the internal, and then the transverse abdominal muscles, were divided to the whole extent of the incision of the skin. After the fascia transversa was reached, it was opened near the inner angle of

the wound, a little exteriorly of the assumed course of the epigastric vessels. A director was insinuated through this opening, and on it the fascia transversa was split from inward outward to the outer angle of the wound. Only, after the subfascial connective tissue had been separated cautiously and gently with the aid of a probe to some depth, gas and fetid pus commenced to escape. The connective tissue was then further separated, first by a uterine sound and then by the finger. Thus the finger entered freely into the cavity of an abscess, situated between the outer surface of the peritonæum and the transversal fascia. It extended, corresponding to the configuration of the swelling felt through the abdominal walls before the operation, behind the recti muscles, beyond the median line to the left. The gaping of the edges of the incision in the fascia transversa caused it at the inner angle of the incision to project in the shape of a sharp falciform spur or fold, in which undoubtedly the epigastric vessels were embedded.

The relief after the operation was immediate and great. The symptoms of general peritonitis rapidly yielded to treatment with opium and iced compresses to the abdomen. The suppuration at once assumed a healthy character. After five days I could cease my daily calls, and, when I saw the boy last, November 11, 1872, thirty-six days after the commencement of the disease and twenty-five days after the operation, the wound was healed, and all the functions of the body were performed in a normal manner. A little resistance at the site of the abscess could still be felt when the examining fingers were deeply insinuated in the abdominal walls. The boy has continued well to this day.

ON A NEW METHOD OF DETERMINING THE POSITION OF ABSORPTION BANDS IN THE SPECTRUM OF COLORED ORGANIC FLUIDS.

By J. C. DALTON, M. D.,

PROFESSOR OF PHYSIOLOGY IN THE COLLEGE OF PHYSICIANS AND SURGEONS, NEW YORK.

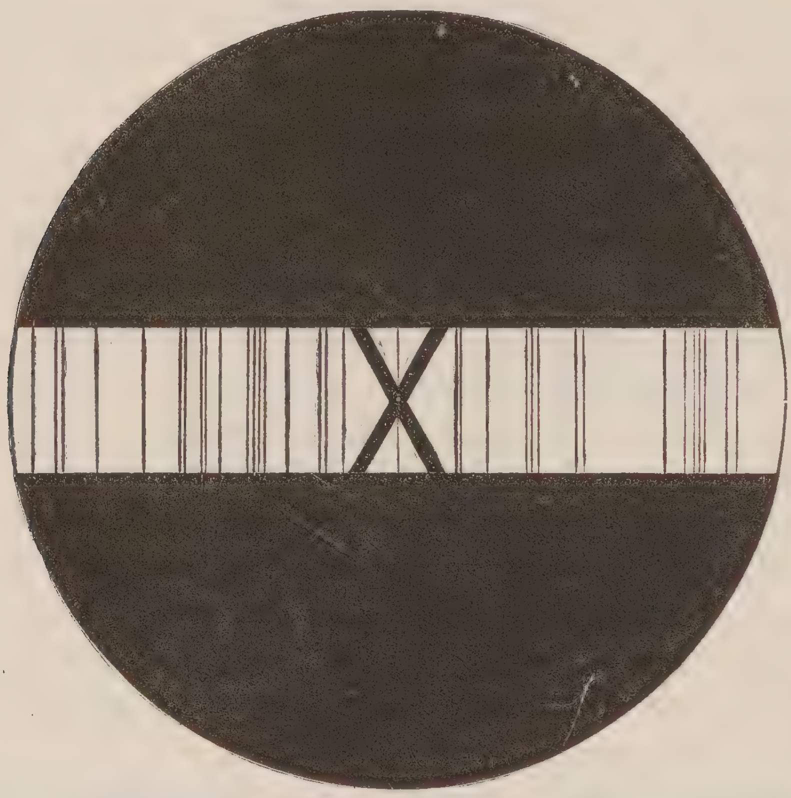
Read October 8, 1874.

THE mode adopted, in many of the spectroscopes in common use, for fixing the location of a spectrum line, is by means of two crossed wires, placed in the eye-piece of the instrument, which may be made to traverse, from right to left or *vice versa*, by the lateral movement of the observing telescope, so as to cover in succession every part of the spectrum. The movement of the telescope carries with it a vernier, sliding upon a horizontal graduated circle, upon which the exact position of the telescope tube, and consequently of that part of the spectrum covered by the cross-wires, may be read off at will. The situation of the principal sun lines, A, B, C, D, etc., having been fixed by observation of the solar beam and duly recorded, appearances in any part of the spectrum may afterward be referred to them as fixed points.

This method is well adapted for astronomical or chemical purposes, in determining with precision the location of the dark sun lines or of the bright lines produced by incandescent vapors; since these lines are of excessive fineness and definition, and any one of them may be brought to correspond accurately, both above and below, with the reëntering angles of the crossed wires (Fig. 1). But it is not convenient for the observation of absorption bands. These bands are very different in character from the delicate, but sharp and mathematically delineated sun lines. An absorption band is simply

a dim space in the spectrum, the central parts of which may be completely black, owing to the total absorption of light, while the edges are almost invariably more or less shaded off, merging gradually into the adjacent illuminated regions. Thus there is no exact point at which we can say with absolute accuracy that an absorption band either begins or ends; and even the position of its centre is necessarily more or less a matter of estimate by the eye. Consequently, the extreme precision of measurement, which is attainable and useful in the

Fig. 1.



Field of the Spectroscope, showing a Part of the Spectrum, with the Sun Lines and Crossed Wires.

case of the sun lines, is neither possible nor necessary for absorption bands; particularly as these bands vary in intensity and extent, and, according to some observers, even a very little in position, with the density of the solutions used for observation.

Beside this, the crossed wires make a coarse and disagreeable object in the field of the spectroscope, and often cover a considerable part of the absorption band which we desire to

examine. Owing to this difficulty, a fine vertical silver wire may be substituted for them with considerable advantage. This appears, in the bright parts of the spectrum, as a single dark line, drawn from above downward, and movable, like the crossed wires, with the changing position of the observing telescope. In the dark parts of the spectrum it may be illuminated by means of a lateral opening in the tube of the eye-piece and a small side mirror, so as to appear as a bright line easily visible by contrast.

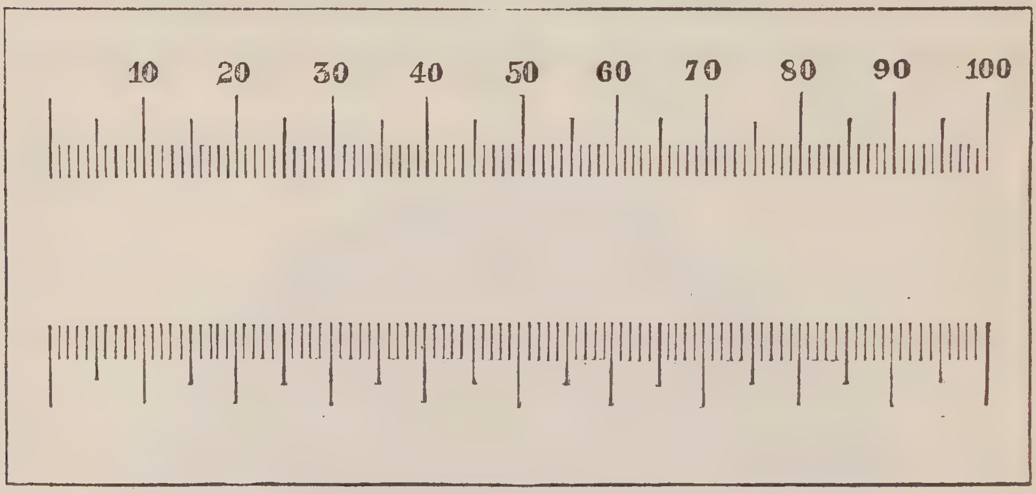
But the necessary calculation of the position of any object in the spectrum, by reference to the vernier and graduated circle, is still an inconvenience. As all diffused light should be excluded so far as possible from the observing room, a gas-light must be turned on and shut off again each time the vernier is to be examined; and when a series of measurements is to be made in rapid succession, the frequent changes required, from the telescope to the vernier and from the vernier to the telescope, are a real source of annoyance and delay. Add to this the calculation in figures necessary for translating the degrees of the graduated circle into the relative position of the sun lines, and it will be seen that the method in question is in no small degree circuitous and troublesome. For these reasons, I have been led to adopt another plan, in which, by a single direct observation, the position of an absorption band may be immediately and easily referred to that of the adjacent sun lines.

For this purpose, a third or scale telescope is used, similar to that employed in many astronomical and chemical spectroscopes, so arranged as to throw the illuminated image of a graduated scale upon the hither face of the prism, from which the spectrum emerges; so that, when both telescopes are properly focused, the spectrum and the scale will be visible together in the field of the spectroscope.

An arbitrary scale is first drawn, with fine black lines in Indian ink, upon smooth, white card-board, about twenty inches long by seven or eight inches wide, with every tenth degree properly numbered, up to 150 or 200, according to the

length of the spectrum in the instrument used by the observer; a blank space being left between the corresponding scale lines, above and below, which is to be occupied by the spectrum. The first half of such a scale, as drawn upon the card-board, is represented in Fig. 2.

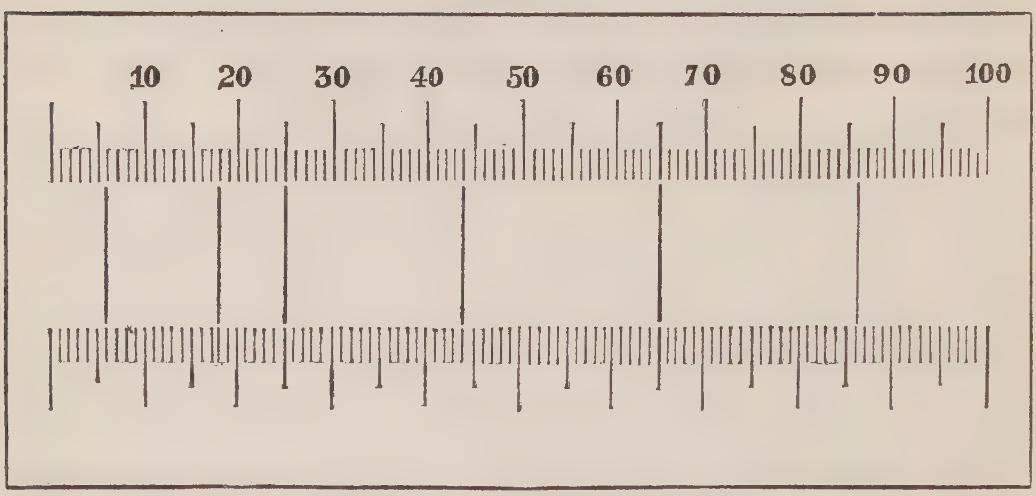
Fig. 2.



Arbitrary Scale on Card-board.

The above scale should then be photographed, in negative, upon a circular glass plate, to be inserted into the end of the scale telescope; the photographed scale to be of such a size

Fig. 3.



Arbitrary Scale, with Sun Lines.

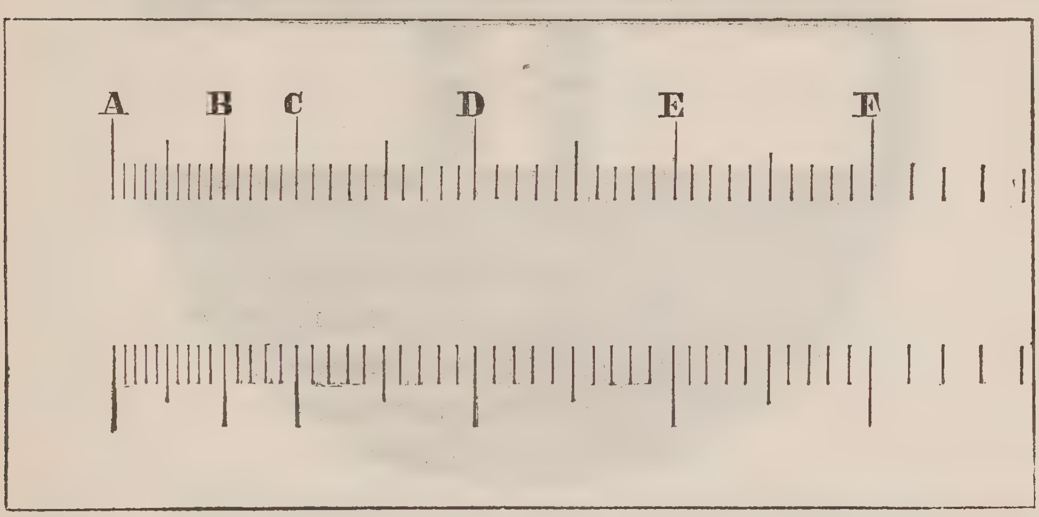
that, when seen in the field of the observing telescope, its image will fairly include the entire length of the spectrum.

With the scale thus fixed in position, a sunbeam is then admitted through the slit of the spectroscope, and the position of the principal sun lines, in respect to the degrees of the scale, accurately noted. Suppose, for example, that the line A comes at 6, B at 18, C at 25, D at 44, E at 65, and F at 86, as in Fig. 3; the line G, in the continuation of the scale, coming at 126, and the line H at 162.

These positions of the principal sun lines being recorded, the photographed scale may then be taken out and thrown away. It has done its work, and is no longer needed.

The next thing is to make a new scale upon card-board, of the same size as the old one, but with the sun lines drawn in their proper position, as ascertained by the preceding observation, and plainly marked with the letters belonging to them. The spaces intervening between adjacent sun lines are divided, each into ten degrees, with the exception of the space between B and C. This interval is so much smaller than

Fig. 4.



New Spectroscope Scale, showing Position of Sun Lines.

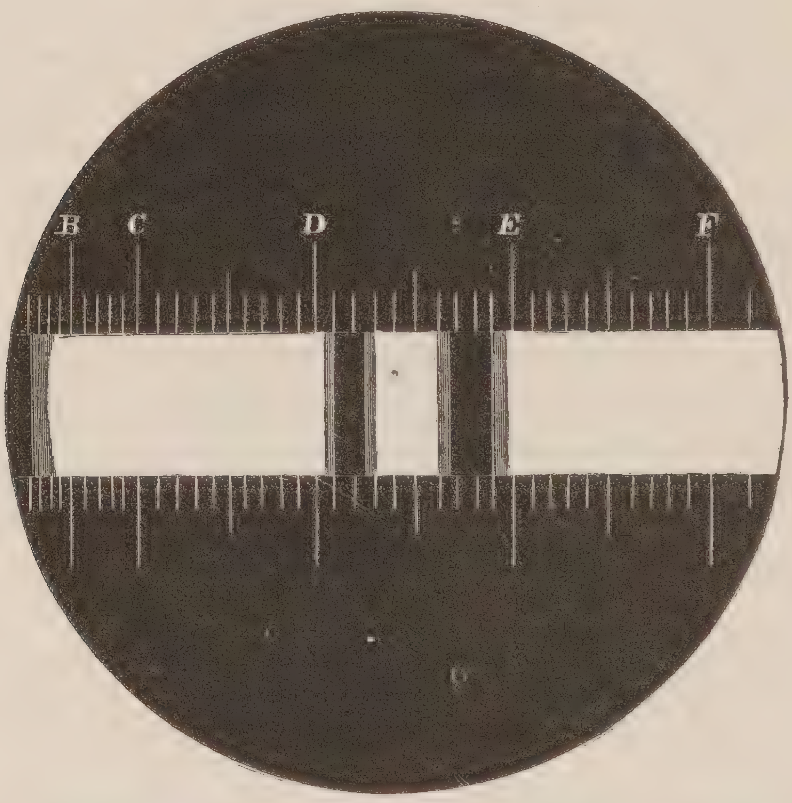
the others that it is more convenient to divide it into five degrees instead of ten. The first half of the new scale will then appear as in Fig. 4.

The degrees into which the intervening spaces are thus marked off will vary in size in different parts of the spectrum; the degrees between D and E, for example, being wider than those between A and B, and those between F and G wider

than those between D and E. But there is no disadvantage in this inequality, since the degrees, in any case, are only for the purpose of properly dividing the space between any two adjacent sun lines, and of enabling us to measure with accuracy one-half, one-quarter, or one-tenth of the distance from one of these lines to the other.

The second scale, when drawn upon card-board in the manner above described, is then photographed in negative upon a glass plate, to exactly the same size as was done with the first; and this second photographed scale is inserted and per-

Fig. 5.



Field of the Spectroscope, showing the Absorption Bands of Blood, and the Spectroscope Scale.

manently fixed in the extremity of the scale telescope. By observation of the sunbeam the accuracy of position of all the lettered lines is verified; and we then have a graduated scale, visible in the field of the spectroscope, by which the position of an absorption band, in any spectrum, by artificial light, may be determined at a glance, without reference to a graduated circle or any numerical calculation. In Fig. 5 is

shown the appearance of that part of the spectrum which contains the two absorption bands of aërated blood, with the corresponding portion of the scale; showing that the first band occupies a little more than the space between the first and the third degree from D to E, the second rather more than that between the sixth and the ninth.

If we express the distance between D and E in hundredths instead of tenths, we should say that the first absorption band of blood extended from 8 to 30 degrees of this distance, and the second band from 60 to 95. The photographed scale may be illuminated at pleasure, by a mirror, from the same burner which furnishes the light for the spectroscope slit.

Three precautions are necessary in using the scale arranged as above:

In the first place, the collimator tube, bearing the slit of the spectroscope, which is usually made movable, must be permanently fastened with screws, in such a way as never to vary its position with regard to the prism of the instrument; for any such variation will alter the apparent length and position of the spectrum.

Secondly, the scale telescope must also be secured from any accidental change of position. It may be made to traverse, in a horizontal plane, by means of a strong spring and thumb-screw; another thumb-screw being placed underneath for the purpose of fastening it, when the proper position has been reached.

Finally, the distance of the photographed scale from the lens of the telescope in which it is fixed must also be invariable. Changing this distance increases or diminishes the apparent length of the scale, as viewed in the observing telescope; and although this does not perceptibly alter the position of the more central lines, as E and F, it does make a difference with those near the extreme parts of the spectrum, as A, B, G, and H. The most convenient distance for use may be fixed at the time of observing the sun lines with the first arbitrary scale, and it should not be afterward changed.

The principal practical difficulty in perfecting the arrangement described above, has been that of obtaining a photographed scale on glass, with the lines sufficiently delicate and the intervening spaces sufficiently opaque; so that the dark background of the scale should not admit any light, to obscure or confuse the spectrum. I am much indebted to Mr. Mason, the photographer at Bellevue Hospital, for the ingenuity and perseverance by which, after many trials, he succeeded in accomplishing this result.

It need hardly be said that the two telescopes, bearing respectively the slit and the scale, should be carefully protected from any mechanical shock or pressure which might endanger the accuracy of their position. Every thing depends upon this position, once fixed, remaining unaltered; and on that account it is well to repeat occasionally the verification of the scale by observation of the sunbeam, or of the sodium line in a candle-flame, which, of course, should correspond exactly with the D line of the photographed scale.

ON ANCHYLOSIS.

By LEWIS A. SAYRE, M. D.,
PROFESSOR OF ORTHOPEDIC SURGERY AND CLINICAL SURGERY IN BELLEVUE HOSPITAL MEDICAL COLLEGE, NEW YORK.

Read October 15, 1874.

Mr. President and Gentlemen of the New York Academy of Medicine:

I shall occupy you a short time this evening in considering the subject of anchylosis, and its treatment.

Anchylosis, from the Greek word ἀγκύλος (*crooked* or *hooked*, because most joints when stiffened are deformed in this manner.) Although the true pathology is stiffness, immobility or consolidation, no matter whether in a straight or crooked position, yet the term anchylosis, or crookedness, has been so long used by the profession to designate the pathological condition of which we are now speaking, that I shall continue to employ it.

Anchylosis is either true, osseous, or complete; or else false, fibrous, or incomplete. True or complete anchylosis signifies the fixed and absolutely motionless state of a joint. False, fibrous, or incomplete anchylosis denotes a limited motion in the joint, no matter how slight that motion may be.

Anchylosis is more common in the ginglimoid articulations than in others, but may occur in every description of joint. In general, only one joint is anchylosed in the same individual; but I have seen one case, in a gentleman under thirty years old, from Providence, R. I., in which both hips, one knee, and both ankles were apparently completely anchylosed, as the result of rheumatic inflammation. I have seen one other case, in a young lad of fifteen, from Kentucky, who had disease of his right hip-joint, and, for the purpose of procuring rest of

that joint, was put by his attending surgeon into a fixed apparatus, embracing the trunk, pelvis, and both lower extremities, and so retained for several months. At the end of this time, the diseased hip was cured by anchylosis, and the knee and ankle of the diseased limb, as well as the hip, knee, and ankle of the oppisite one, were completely anchylosed, and still remain in the same condition.

In this case there had been no inflammatory action in any of the joints, except the right hip, and he had never complained of or suffered pain in any of them. This case is of great importance, showing as it does that anchylosis can take place even in a young person, in a perfectly healthy joint, by long-continued rest.

In old age, anchylosis, in certain parts of the skeleton, is a natural change ; and in this period of life it is common to find the heads of the ribs anchylosed to the bodies of the vertebræ, or the tubercles to the transverse processes, the vertebræ to one another, the ensiform cartilage to the sternum, etc.

Anchylosis is not a disease of itself, but may be the result of any disease, affection, or injury, which interferes with the normal functions and motions of a joint.

Anchylosis may be the most favorable termination that can occur in many diseases and accidents of the joints. In such cases it is of the most vital importance that the surgeon should select the most favorable position for the future usefulness of the limb thus involved. As, for instance, the elbow is more useful when anchylosed at a right angle than if made straight, whereas the knee would be entirely useless if anchylosed in the same manner; its future usefulness and security being better obtained, by having it anchylosed perfectly straight, or as nearly so as may be. It is owing to the neglect of observing this principle of placing a limb in its most favorable position for future usefulness, while consolidation is taking place, that subsequent surgical interference is necessary.

In chronic or long-continued inflammation of any joint, reflex irritation, producing muscular contractions, invariably takes place.

This contraction not only aggravates the disease by causing undue pressure on the parts inflamed, but also distorts the limb in accordance with the action of the most powerful muscles involved, and the distortion can only be prevented by the proper application of an extending and counter-extending force during the treatment of the disease. When this principle has been neglected, the patients frequently recover with such seriously distorted and useless limbs as to render surgical interference necessary.

In such cases it is of the utmost importance to ascertain whether the anchylosis be complete or incomplete, as the plan of treatment in each particular case depends entirely upon the accuracy of this diagnosis. If fibrous, or incomplete, it can be broken up by manual or mechanical force, aided by subcutaneous tenotomy, myotomy, and the section of such fasciæ, fibrous bands and other adhesions, as have prevented its mobility; whereas, if the anchylosis be true, or bony, the deformity can only be relieved by section of the bone itself with the saw or other instrument. In many cases of simply fibrous or incomplete anchylosis, the adhesions are so firm and so short as to allow of no perceptible motion, even under a very careful inspection. In such cases, if there has been any motion whatever, although so slight as not to be observed at the time, yet on the following day the parts which have been subjected to the violence necessary for the examination will give evidence, by pain, tenderness, and inflammation, that some motion must have been given to the parts involved. In one case of anchylosis of both hips, with very great distortion, by complete flexion and adduction, in a young girl of nineteen, from long-continued suppuration of both hip-joints, the anchylosis was so complete that, in consultation with all the surgeons at Bellevue Hospital, we all decided that it was a case of osseous fusion, and could only be relieved by section of the bone.

On the following day, when I went to perform the operation, there was so much tenderness about the parts, that I was satisfied some motion had been given to the articulation, although so slight that none of us had been able to detect it at

the time of the examination. I therefore determined to break up the adhesions, instead of sawing out a portion of the bone. The adductors tensor-vagina femoris, and fascia lata, of both sides were subcutaneously divided, the wounds carefully closed and covered by long strips of adhesive plaster and compresses. A figure-of-8 roller was then carefully applied around each hip, after which the adhesions were forcibly but very freely broken up, and the limbs brought as nearly as possible to their natural position, and retained there, by extension and abduction by weights and pulleys, which were secured to the limbs, in the usual way, by adhesive plaster and roller. The patient was kept perfectly quiet, the parts kept cool with ice-bags, and at the proper time passive motion was made. The result in this case was perfectly satisfactory, the patient recovering, with good motion of both joints. She has married since, and was delivered by the late Dr. George T. Elliot, of a living child, who is now a robust boy, of five years of age. Previous to the operation, this woman could only walk upon her hands and feet, the limbs being closely flexed and adducted, and the anchylosis so complete, as before stated, that all who examined her thought it to be osseous. She is now in perfect health, and performs all her household duties without the aid of a servant.

Having made our diagnosis that the anchylosis is fibrous, and not osseous, how shall it be broken up? In former times gradual extension, with steaming and friction, was considered all that was necessary, but the length of time demanded and the great pain induced by this method of treatment, frequently prevented the patient and surgeon from carrying it to the completion of securing perfect motion. The slow and gradual stretching of tissues, long contracted, produces reflex contractions in many instances to such a degree as to compel the treatment to be abandoned, and patients prefer to remain with their limbs in the distorted condition rather than undergo the constant pain of continued extension.

In all such cases it is infinitely better to proceed to the immediate restoration of the joint to its normal position, with

entire freedom and mobility by manual force under the influence of an anæsthetic combined with tenotomy or myotomy or subcutaneous section of the fascia, if necessary, than to resort to the slow process of gradual extension.

How are we to decide whether tenotomy, myotomy, or the section of fascia, is requisite? By putting the parts upon extreme tension, and while thus stretched, if point pressure by the finger or thumb be made on the fascia or tendon thus stretched produces reflex contractions, then that fascia, tendon, or tissue, must be subcutaneously divided or else forcibly ruptured before the limb can be restored to its normal position. If the tissues thus contracted can be reached with the knife without the danger of involving large blood-vessels or nerves, section by the knife is better than forcible rupture. If it is necessary to make this subcutaneous section, it is better to do it three or four days previous to the breaking up of the joint, so that the external wound made by the tenotome may have adhered before the latter operation is performed. This tenotomy may be performed under the influence of an anæsthetic, or not, as the surgeon chooses; but when the *brisement* proper is performed an anæsthesia is absolutely essential. In fact, it is due to anæsthesia that *brisement forcé* has gained its reputation, and to it chiefly owes its success.

The patient being thoroughly anæsthetized, the limb is seized by the hands of assistants, holding it with firmness, between the joint involved and the trunk, while the surgeon takes the farther extremity of the limb and forcibly flexes it upon itself, which is frequently attended with sharp snaps and cracks that are sometimes quite audible and that are very distinctly *felt* by the surgeon's hand while making the rupture. Having flexed it sufficiently to begin to allow of moderate movements, he then reverses the movement and forcibly extends it; and in this way, by forcible flexion and extension, continues until he has gained perfect and free motion of the joint involved in all its normal movements. If the knee is the joint involved, care must first be taken to fracture off the patella from its attachment to the femur, which is some-

times the most difficult part of the operation to be performed. In many instances a surgeon can aid himself by covering the handle of a key with buckskin, and by its use give himself a firmer leverage against the edge of the patella than he can get with his naked thumb. Having thus obtained a perfect extension, and perfect flexion, in fact, the complete movements of whatever joint involved, these movements are repeated with great freedom and with great frequency until all the adhering surfaces are thoroughly and completely broken up.

One of the commonest causes of failure in the treatment of *fibrous anchylosis* by *brisement forcé* is, that the surgeon, succeeding in getting a moderate motion, and becoming alarmed at the audible fractures that occur, contents himself with that slight motion for the present operation, intending to complete the cure by subsequent operations, and thus, by making frequent attempts to increase these slight movements, sets up a new inflammation in the parts involved, preventing any further interference, and frequently resulting in a more firm consolidation of the joint than before; whereas, by breaking up the adhesions thoroughly and completely at the time of operation, and then, by proper dressings of the parts and the prevention of inflammation, he may confidently expect that he will have a much more satisfactory result.

How are these dressings to be applied? and how is this inflammation to be prevented? This I look upon as the most important part in the treatment of an anchylosed joint. For many years past I have always adopted the following plan: If, for instance, it be the knee which I have broken up for *angular fibrous anchylosis*, I first strap the toes with strips of adhesive plaster if it be a small subject, or if an adult with long toes, pad the toes with cotton and bind with bandage, carrying the roller over the foot strongly and firmly, padding the malleoli and tendo-achillis with cotton, the roller is carried snugly over them; two strips of adhesive plaster having been placed on either side of the leg for extension, the roller is passed over them, leaving their lower extremities exposed for the future attachment of weight and pulley, and is

carried up as far as the top of the tibia. The popliteal space is then padded and firmly strapped with strips of adhesive plaster, each one shingling over the other until the entire knee is covered. The roller is then continued over the knee smoothly and very firmly until you come to the junction of the middle and lower third of the femur, when a piece of sponge an inch or two in length, and about the size of your thumb, is placed over the track of the femoral artery, and the roller carried on over this sponge for the purpose of making partial compression of this artery, so as to diminish its calibre and thus prevent the full supply of blood to the parts below. Great caution is necessary in the application of this pressure upon the artery not to obstruct the circulation so as to produce gangrene; we must here *use* pressure without *abusing* it. The limb is then secured in an absolutely immovable position either by a wooden splint well padded placed behind the leg, gutta-percha, sole-leather, plaster of Paris, iron bars on either side of it, or in any way that the surgeon may deem best for the purpose of preventing the slightest possible movement. The patient is then placed in bed, the lower extremity of which is raised ten or twelve inches higher than the head of the bed, so that the body may act as a counter-extending force, and the weight and pulley applied over the foot of the bed to the strips of adhesive plaster at the ankle-joint before described. Ice-bags are then placed around the knee, and such constitutional treatment in the way of narcotics, cathartics, etc., as may be required are judiciously used. At the end of six or seven days the dressings are removed, the sponge taken from over the femoral artery, the adhesive straps cut from over the knee, and the parts carefully examined, and a very slight movement given to the joint for the purpose of preventing solidification, when the dressings are reapplied with the sponge left off from over the femoral artery. At this dressing the surgeon will often be surprised to find ecchymosis to some extent, both above and below the joint, from extravasated blood caused by the rupture of vessels at the time of the operation; but, by following the plan that I have here laid down, I have

never seen a case that went on to suppuration since I have adopted this method of treatment, now numbering nearly one hundred cases. The extension is still continued and the elevated position of the limb is still preserved for some days, until all danger of inflammation is passed, the surgeon exercising his judgment whether the application of ice is still to be kept up or not. At the end of a few days the dressings are again removed, and more free motion is given to the part. It may be necessary at the time of making this movement, and the three or four subsequent movements, to administer an anæsthetic; these movements should be made quite free when an anæsthetic is used, the surgeon being careful not to carry them to the point of exciting new inflammation. After some days the passive movements can be made daily, accompanied with friction, and shampooing should be very liberally done. These movements may be increased in frequency as the case advances, until finally an instrument can be so adjusted to the limb that the patient can cause the movements many times in the day without the attendance of his physician. So soon as the parts can be pressed together by bearing the weight of the body upon the foot without tenderness, the extension can be omitted, and the movements daily increased. By pursuing this plan, and by the application of the same principle to the wrists, elbows, and other joints, I have never had a case of constitutional fever or suppuration follow the *brisement forcé* of any joints.

In cases of *complete* or *bony anchylosis*, section by the saw is absolutely necessary. Barton, of Philadelphia, first made section of the upper portion of the femur for *angular contraction* with *bony anchylosis* in 1826, with a very perfect result. Dr. Gurdon Buck, of this city, performed the same operation in the New York Hospital in 1841 or 1842, by taking out a V-shaped portion of the knee for *angular anchylosis* at that joint. I modified Barton's operation for anchylosis of the hip-joint in 1862, by making a curved section of the femur above the trochanter minor and a straight section a few lines below the first curved cut, thus removing a block of bone.

This operation I made in two cases, and both resulted in perfect success. The first case is still living. The other case died of another disease some months after the operation, but lived long enough for Nature to make an entirely new joint with capsular ligament, synovial membrane, and a double ligamentum teres, which is here seen in the specimen before you. These cases have all been published, and therefore there is no necessity of making any further mention of them.

Mr. Adams, of London, has very much simplified this operation by making a simple subcutaneous single section through the neck of the femur in these angular deformities of the hip, with very satisfactory results. Dr. Sands, of this city, has repeated Dr. Adams's operation, with the result of a movable joint. Reasoning *a priori*, I would suppose that the single section through the bone, although you might by it remove the deformity, you would be in danger of effecting a cure by anchylosis. The case of Dr. Sands, and some of those reported by Dr. Adams, seem to disprove this position, but sufficient time has hardly elapsed to judge whether they may not after a while become anchylosed, although in an improved position.

CASE I.—*Anchylosis of Knee; Brisement Forcé; Result perfect, Ward* 8, *Bellevue Hospital; from Hospital Records.*

"R. D. Steele, June 29, 1869, aged twenty-two, Kentucky. On the 11th of December last, patient accidentally shot himself with a Colt's revolver, the ball entering the right thigh, on its anterior aspect, midway between the groin and the knee.

It lodged in the tissues, on the outer side of the patella. The next day the ball was removed. Patient says that his knee then began to inflame, getting swollen, red, and painful.

There was much discharge through the opening made by removing the ball, and patient was confined to his bed for two months.

During this time his knee became anchylosed, almost in a straight line.

On admission to hospital, the right thigh and leg were smaller than left, the following measurements being taken: Right thigh, 15⅝ inches in circumference; left thigh, 17⅛ inches in circumference; right leg and calf, 10½ inches in circumference; left leg and calf, 12¼ inches in circumference.

There is barely any motion of the joint. The patella is slightly movable.

Patient's general condition good. He gives no history of hereditary disease; the limb gives him no pain.

June 30*th*.—To-day patient was etherized, and Dr. Sayre broke up the adhesions with little trouble, so that the leg could be completely extended and flexed, at an acute angle, upon the thigh. The toes were strapped, the foot and leg bandaged, a large sponge strapped into the popliteal space, and another placed over the femoral artery, so as to compress it moderately. A long splint of leather was then adapted to the back of the thigh and leg, and bandaged firmly. 7 P. M. patient doing well; has some pain; ordered liq. morphiæ sulph. (U. S. P. ʒ iij).

July 1*st*.—Slept well last night, and has no pain in knee. 7 P. M., foot rebandaged.

6*th*.—Since last note patient has been doing well. Today Dr. Sayre took off the splint and bandage, and made passive motion, which was very painful. Patient was then anæsthetized, free passive motion made, and dressing reapplied.

7*th*.—Joint was moved again.

9*th*.—Splint removed to-day. Patient out of bed.

14*th*.—Joint moved to-day under chloroform. From this time the motions were made more frequently, and an instrument adjusted, so that the patient could flex and extend the limb at his pleasure. He was advised to do this frequently every day. The result was, that he recovered with perfect motion in less than three months.

I saw Mr. Steele in January last, and his limb is as perfect as the other."

Case II.—*Necrosis of Lower End of Femur, complicated with Fibrous Anchylosis of Knee-Joint; Brisement Forcé; Recovery with Motion.*

George W. Orr, of Bloomingdale, aged twenty-four years; fell, when he was ten years old, from a height of ten feet, striking upon his right limb, followed by a periostitis of the lower end of the femur, ending in necrosis of femur and anchylosis of knee-joint. When he was fifteen years of age (after a lapse of five years), one of the sinuses of the outer portion of the thigh was dilated, and a piece of bone two and a half inches in length and about two-thirds of the circumference of the femur was removed. His leg at that time was flexed at an acute angle with the knee.

The wounds of the thigh healed after a few months, when, under the influence of chloroform, by *brisement forcé*, his limb was made perfectly straight, dressed in my usual way with a partial compress over the femoral artery, retaining splint, binding the knee, extension by weight and pulley, ice-bags to the knee-joint. No constitutional or other irritation followed the operation. At the end of seven days the dressings were removed. Considerable ecchymosis appeared round the neighborhood of the knee from the rupture of blood-vessels at the time of the operation, but no excessive heat or other evidence of inflammatory action. The limb was very slightly moved and again redressed as before, with the exception of the sponge compress upon the femoral artery. In two days it was again redressed and more free movements given it.

From this time on, the dressings and motions were made daily for about a fortnight, when the passive movements were advised to be made several times within the twenty-four hours. These movements were constantly increased, until, at the end of three months, the cure was perfect and complete, with the entire mobility of the joint, complete extension and perfect flexion, as is now seen in the case before you.

Case III.—*Fibrous Anchylosis—Knee.*

Joseph S., aged seven years, was brought to me October 30, 1873. The following scanty history of the case was all that could be elicited:

When two years old he had rheumatism. The joint chiefly affected was the left knee. The father says, "His physician called it '*bony anchylosis*' and '*white swelling*.'" It was treated with iodine externally—no extension. The limb was always crooked, but he could walk upon it until the summer of 1872, since which time the present distortion has existed. There is fibrous anchylosis of the knee. The tibia is luxated backward. There is very slight motion of the joint; the patella is probably movable.

Treatment.—December 6, 1873. At the college clinic I divided the hamstring tendons of the left limb subcutaneously without loss of blood. The patella was then forcibly separated from the end of the femur and the limb drawn down to the position of complete extension, and retained by a weight-and-pulley dressing. The limb was dressed in my usual manner, viz.: The instep and ankle were well padded with cotton, the roller neatly applied over this and up the leg. The popliteal space is protected by a large soft sponge. The inequalities of the knee carefully padded, strips of strong adhesive plaster are snugly drawn over the sponge and pad, and the whole covered by carrying the roller up over the knee and lower part of the thigh. A small piece of sponge is then placed over the course of the femoral artery, above the junction of the middle and upper third of the thigh, and the roller carried farther up and completed by a spica. The boy was taken directly to lodgings, put to bed, and a dose of morphia given him.

11*th.*—Dressing removed and reapplied. Most excellent condition in every way.

20*th.*—Came to clinic with extension-brace, which was applied yesterday. Motion good; passive motion ordered. Returned to his home in Yorkville.

January 12, 1874.—Has for a week past complained of pain, particularly for the past three days. Compression in axis of limb gives pain. Extension gives relief. Knee-extension instrument ordered.

April 1*st.*—Boy walked into my office without crutches.

Instrument readjusted. Suffers none from knee, but has symptoms of "chills and fever." Lives near the "Vanderbilt improvement," Ninetieth Street. Ordered quinine and iron.

June 1*st.*—General condition good; still tender over lower insertions of lateral ligaments. Instrument readjusted.

23*d.*—Boy doing well. Instrument not removed, but bandage reapplied.

August 1*st.*—Instrument removed; walks well, with good motion, about one-third normal freedom.

CASE IV.—*Fibrous Anchylosis—Knee; sub-luxated; Brisement; Recovery.*

William M., aged nine years, from Auburn, N. Y., came to me November 5, 1868, and gave the following history: About June, 1864, the father noticed that the lad began to drag his left foot. He complained of no pain, and appeared to have nearly perfect power over the limb. Five or six months later the knee began to swell, and appeared to be "*filled with water.*" This condition continued for about two years. Gradually the swelling disappeared. He was treated at intervals during the continuance of the trouble, by various physicians. About two years since the child was ordered to go upon crutches, no attention being paid to the contraction. The limb was then nearly straight, but since that time the contraction has gradually increased. For the last eighteen months the limb has been nearly as "tough and sound" as the healthy one, saving the contraction.

The position of the limb is as follows: The leg is sub-luxated backward and outward slightly. There is slight motion at this new joint. The patella is apparently adherent by bone.

6*th.*—Drs. Hamilton and Krackowizer saw the patient with me. While examining the patella, Dr. Hamilton thought he detected motion. This was rendered certain by the following manœuvre: Dr. Hamilton placed his finger upon the groove between the patella and external condyle, so that the sharp edges of the two bones could be at the same time felt. I then made firm pressure upon the inner edge of the patella, and the two edges of bone before mentioned were felt to ap-

proximate, the patella slightly overriding the condyle. The opinion of the consultation was, that an attempt should be made to restore the normal position of the limb; that, under anæsthesia, as much as possible should be done, and the limb retained in the position gained, by a splint, or by extension, according as might be best in practice.

9th.—The boy was chloroformed, and the limb forcibly straightened as far as possible. While the limb was firmly held in proper position, a weight-and-pulley extension was applied. The vessels were protected by a sponge in the popliteal space. The supply of blood to the joint was diminished by the pressure of a small sponge placed over the femoral artery and confined by the bandages.

27th.—Made a second operation. At this sitting the limb was brought nearly straight, the hamstrings were probably broken. The limb was fixed by a posterior leather splint. No reaction of importance followed.

December 19th.—The limb is nearly in perfect line. Passive movements have been employed for two or three weeks.

Put on an instrument for angular motion of the knee. The boy left for home; treatment to be continued under direction of his family physician.

February, 1869.—The father writes: "The knee had improved very much, when the boy was seized with typhoid fever and died."

Case V.—*Fibrous Anchylosis of Left Knee.*

Catharine B. was admitted to Bellevue Hospital June 3, 1868, when she gave the following history (copied from hospital register): She was confined April 1, 1868, and remained in bed but two days after. On the 13th of April she first noticed pain in the left leg and knee. Very soon the parts became much swollen, red, and very tender; at the same time she had chills, fever, and sweat. She was compelled to keep the bed for four weeks. Since her admission to the hospital, the knee has been blistered, compressed with sponges, and extended. All these plans seemed to be of some benefit. Passive motion, showering with alternate hot and cold water,

have been employed with little effect upon the anchylosis. For several months the joint has been anchylosed, the angle being about thus  —135°. There is at present but little pain in the knee, though she says "it is worse in damp weather."

At clinic, January 6, 1869, I made the following comment on the case, previous to operating: The hectic sweats, etc., lead to the belief that this was a case of pyarthrosis, but the liquid has since been nearly all absorbed, and it was probably all synovia. The anchylosis is at too great an angle, and I shall therefore try by *brisement* to place it in a better position. The patella seems to be movable. There is some danger of reëxcitation of inflammation by the *brisement*, since hitherto all attempts at establishing motion have been attended with considerable reaction. There is one point, below and outside of the patella, which is still tender. "I do not hope in this case to get motion." I applied the preparatory dressings as usual. The patella was started off by bending the limb backward, and then straightening it. Free motion was given to the joint. The knee had become slightly inverted; this was straightened by pressure. The usual dressings of sponges and plaster and roller, with a posterior splint, were applied. Directed absolute rest for ten or twelve days.

13*th*.—No reaction took place. Every thing proceeding perfectly well. Patient has had no pain after the first thirty-six hours succeeding the operation.

20*th*.—Extension no longer giving relief, was removed as unnecessary.

February 13*th*.—Has continued to do well.

May 1*st*.—Has continued to improve; is walking with the aid of a stick.

14*th*.—Having left the hospital on a pass, and overstaid her time, she was discharged to-day.

Case VI.—*Anchylosis—Hip.*

Miss ——, of Hudson, N. Y., was brought to me by Dr. J. F. Phillips, of Claverack, N. Y., November 27, 1867, giving the following history :

When three years old she caught her foot in a hole and fell. She was able to walk home, but complained of severe pain, and was confined to the bed for two years from that time. During this time the right lower limb became strongly flexed on the pelvis and adducted across the upper portion of the opposite thigh. Previous to the injury she had been perfectly healthy.

Since she was five years old she has been able to go round on crutches, and for the last six or seven years has been able to *flex* the thigh upon the pelvis and extend it slightly, but cannot *abduct* it at all.

General health perfect, and tolerably robust. Right limb five inches shorter than the other; that is, the foot cannot be brought within five inches of the floor (when the sound limb is straight), and it is very strongly adducted.

A line drawn from the right tuberosity of the ischium around the hip, to the anterior superior spinous process of the ilium of the same side, passed nearly *three inches below* the top of the trochanter major, which could be distinctly felt on flexion and extension of the thigh upon the pelvis, showing that a *new joint* had been made upon the dorsum of the ilium, but on account of the adduction of the limb she could bear no weight upon it without falling upon the right side.

I put her under chloroform, and, by moderate force with my hands, very slowly and gradually abducted the limb, Dr. Phillips holding the pelvis quiet, when suddenly the tendon of the adductor longus snapped off with quite a loud noise. After a few minutes I was able to *abduct* the thigh to nearly a right angle with the body, the pelvis being held still and the other limb being straight, showing that the motion was in the new hip-joint and not in the lumbar region. The recovery from chloroform was slow, but at the end of two hours she could rise and walk with the limb straight under her. She could *voluntarily* abduct the limb six inches from the central line of the body. It was now only two inches shorter than its fellow, and could nearly support the weight of the body.

The patient returned to Hudson on the same day in a

sleeping-car, without experiencing any trouble, having been carefully bandaged on a well-padded board, and on reaching home was put to bed and fomented.

December 1st.—I saw her; found her perfectly comfortable, and she had suffered no pain since the operation. There was a slight discoloration upon the inside of the thigh. She is able to *flex*, *extend*, and *abduct* the limb and to bear her entire weight upon it without pain if she has gentle support to prevent her falling, the muscles not being strong enough to sustain or steady her body.

I directed that the limb should be rubbed, shampooed, and that Faradism should be applied to it.

12*th.*—Dr. Phillips reports, "Case still improving."

September, 1868.—Miss —— called upon me. The limbs are parallel. The limb formerly anchylosed can now be moved voluntarily in every direction, and over quite a large arc. The knee of the diseased side is considerably above that of the sound side. The right limb, measuring from the top of the trochanter major to the external *malleolus*, is one inch shorter than the left. This shortening is increased by the position of the head of the femur, so that, measuring from the anterior superior spinous process to the internal malleolus, the shortening is two and a half inches. The discrepancy is made up by a thick cork-sole, and she walks well with the assistance of a cane.

CASE VII. — *Fibrous Anchylosis of Hip; Tenotomy; Brisement; Recovery, with Motion.*

G. W. S., aged fourteen years, consulted me for the first time, September 17, 1872, and gave the history of his case as follows:

Nearly ten years before, he was attacked with hip-disease on the left side, as the result of a fall. The trouble continued for five years, during which time the disease progressed to the third stage, abscesses formed, were opened and discharged, small pieces of bone coming away from time to time. No large pieces have ever been discharged.

About five years from the beginning of his trouble, while

running, he caught and twisted his foot in a rope. For several weeks afterward he was unable to move without the greatest suffering. He subsequently improved, and became quite sound and strong.

Health good. Wears, in walking, *four and a half inches lift* upon the left shoe. He is not easily fatigued in walking, and does not complain of pain. When his trunk and the sound limb are in normal position, the affected limb is flexed and *abducted*, the left foot falling upon the outside of the right knee. It is brought down to a position permitting walking by strong tilting of the pelvis. It is possible that the second accident, above mentioned, may have increased the motions of the joint.

28*th*.—Under chloroform, I divided subcutaneously the tendons of the adductors (pectineus, adductor-longus gracilis) and the tensor vagina femoris; dressed the usual way, and placed in the wire-breeches.

October 12*th*.—No inconvenience has been experienced by the patient. He was removed to-day from the wire-breeches.

19*th*.—Was allowed to ride out.

December 6*th*.—Has had a two-inch lower-heeled shoe constructed; called to-day to show it. Walks very well with it; the limbs are parallel, and he is able to flex the thigh upon the pelvis to a right angle.

SPINAL PARALYSIS OF THE ADULT; ACUTE, SUBACUTE, AND CHRONIC. (INFLAMMATION OF THE MOTOR TRACT OF THE SPINAL CORD.)

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Read November 5, 1874.

Mr. President, and Fellows of the Academy:

I desire in this essay to call your attention to a rare form of myelitis, one whose existence has been revealed to us only within the last few years, by the progress of pathological anatomy and physiological analysis. The few observers who have recorded cases of this disease have given it different names, the chief of which are as follows:

Acute anterior spinal paralysis (Duchenne).

Subacute general anterior spinal paralysis (Duchenne).

Spinal paralysis of adults (Meyer, Charcot, Gombault).

Myelitis of the anterior horns (Dujardin-Beaumetz).

Acute spinal paralysis of adults (Petitfils).

An affection of adults like infantile spinal paralysis (M. Bernhardt).

I prefer the names of acute, subacute, chronic spinal paralysis of adults; or, inflammation of the motor tract of the spinal cord. I have adopted the latter name for reasons which will be fully given when I come to discuss the pathological anatomy of the disease. The first name is a semeiological denomination, the second a pathological one; both being useful in the early stage of our knowledge of a disease, the second being the one which should remain as a part of nosology.

Before relating the cases upon which this paper is based, it were well to give a short historical sketch of the subject.

Duchenne[1] (de Boulogne), in the last edition of his work on electrization, claims that he recognized the subacute form of this affection as early as 1847; and that in 1853 he gave to the profession the first sketch of its symptoms. Concerning the acute form, he writes (p. 437) that he "had long thought that the symptomatology of the atrophic palsy of childhood, whose anatomical character is mainly acute atrophy of the anterior cells of the spinal cord, was not met with in adults; but that having met with the same symptomatology in the latter, I have naturally concluded that this paralysis must be produced by the same lesion. This led me to give this palsy the name of acute anterior spinal palsy of the adult, or palsy by atrophy of anterior cells." Moritz Meyer,[2] in his work on medical electricity, describes spinal palsy in the adult, and recognizes its analogy with infantile spinal paralysis, giving two interesting cases. As early as 1863, Prof. Charcot,[3] of Paris, was consulted by a patient bearing the remains of an attack of acute spinal paralysis. Charcot at that time expressed the opinion (in his notes) that the case was like the cases of infantile spinal palsy: "If we leave out the condition of age, the case of Mr. L. might be by all its characters likened to the myopathic paralysis of children." Speaking in 1872, Charcot[4] says: "With reference to the *spinal paralysis of adults*, and the *general spinal paralysis* (Duchenne), we have as yet no definite teaching from pathological anatomy. Judging by their symptoms, it is at least very probable that these affections are connected with a lesion of motor nerve cells.

[1] Duchenne, *De l'Électrisation localisée*, 3me édition, Paris, 1872, pp. 437, 438, 459–461.

[2] Moritz Meyer, *Electricity in its Relations to Practical Medicine.* By Dr. Hammond. New York, 1869, pp. 229–242. (German third edition, 1868.)

[3] Charcot's private notes cited by Petitfils, *Considérations sur l'atrophie aiguë des cellules motrices.* Paris, 1873, pp. 72–77.

[4] Charcot, *Leçons sur les maladies du système nerveux*, Paris, 1872–'73, p. 63.

The spinal paralysis of adults recalls that of children by the almost sudden accession of palsy, by the tendency to retrocession, which is shown at a given moment, by the quickly-developed loss of electro-muscular reaction to the faradic current in the palsied muscles, and lastly, by the rapid atrophy which these muscles undergo." Dujardin-Beaumetz,[1] in his aggregation thesis, indorses Duchenne's views in regard to the semeiology and probable pathological anatomy of this disease. Gombault,[2] in the early part of 1873, published a case of acute spinal palsy of the adult with *post-mortem* examination. He found lesions in the spinal cord which were those anticipated by Duchenne, Charcot, and others. Upon this excellent observation our view of the pathology of these cases rests. Charcot's cases and Meyer's were republished in 1873, in a thesis by Petitfils.[3] During the present year the only contribution to the literature of the subject has been an article by M. Bernhardt,[4] of Berlin, based upon one case without autopsy. In this country the subject is mentioned in an appreciative way by Dr. Mary Putnam Jacobi [5] in a paper, remarkable for erudition, upon paralysis in childhood, published this year.

For my own part, I can say that in November, 1871, I clearly recognized the affection as illustrated in Case XVIII. This girl, with her wasted left leg, I often pointed out to the resident staff and to visitors as an example of infantile spinal palsy in the adult. Last spring I had occasion to lecture clinically upon a man affected with this disease, and then pointed out to the class the nosological place of acute and subacute inflammation of the anterior motor tract of the spinal cord.

[1] Dujardin-Beaumetz, *De la myélite aiguë.* Paris, 1872, p. 51.

[2] Gombault, Note sur un cas de paralysie spinale de l'adulte suivi d'autopsie. *Archives de physiologie normale et pathologique*, 1873, pp. 80–87.

[3] Petitfils, *op. cit.*, pp. 71–83.

[4] M. Bernhardt, Ueber eine der spinalen Kinderlähmung ahnliche Affection Erwachsener. *Arch. f. Psychiatrie u. Nervenkrank.*, 1874, p. 370.

[5] Pathology of Infantile Paralysis. *American Journal of Obstetrics and Diseases of Children*, May, 1864, p. 21. [Read before the New York County Medical Society, December 22, 1873.]

The notes of this patient's condition have been mislaid, and his case is therefore omitted from the essay.

I shall first give abstracts of the few cases of acute and subacute spinal palsy which I have found recorded; giving full details only of Gombault's case with autopsy. I shall then relate my own cases, and afterward consider the semeiology of the disease—its diagnosis, its pathological anatomy, its etiology, its prognosis, and its treatment:

Duchenne's Cases, under title of *Acute Anterior Spinal Palsy.*

Case I. (Obs. LXIX.)—Female, aged twenty-two. Fever; pain in muscles, and down vertebral column, severe in cervical region; numbness in fingers, loss of voluntary movements. Sensibility preserved; urination and defecation done normally. Fever ceased on fourth day, leaving palsy of all limbs, which disappeared spontaneously. Over six months after, patient showed palsy, with atrophy of right tibialis anticus, right deltoid, infra-spinatus, biceps, interossei, and thenar muscles, and the left serratus magnus and flexor muscles of the fingers. Improved by localized faradization.

Case II. (Obs. LXX.)—Male, aged forty-two. In 1848, an effort accompanied by cracking sensation in back, and followed by severe pain, extending into the limbs, with numbness in, and, later, palsy of both lower extremities. Paraplegia cured spontaneously, in two months. In 1869, without cause, fever and general paralysis. Rapid wasting of forearms and hands. Wasting of some muscles of lower limbs. Improvement in four months. When seen by Duchenne, showed atrophy of hands, of extensors of wrists, and of anterior tibial muscles. At no time any bladder or sphincter ani palsy.

Case III. (Obs. LXXI.)—Male, aged twenty-one. Lay on snow while naked, and fainted away. Then had chill, fever, and delirium, with consequent general paralysis. Sensibility perfect; no trouble with bladder or sphincter ani. When seen (fifteen years later), showed atrophy of all right-leg muscles, and wasting of nearly all muscles of left leg.

Upper extremities exhibit wasted deltoid and hand-muscles on right side, and serratus magnus on left side. Sensibility a little dull in hands and feet.

Case IV. (Obs. LXXV.)—Female, aged eighteen. Hard work out-of-doors. Fever lasting eight days, with pain in cervical spine and limbs, and numbness in latter. Palsy of both arms and of legs, with preservation of sensibility; bladder and intestines normal. Residue, atrophic palsy of right upper extremity. Improvement in palsy spontaneous, in second month.

Duchenne's Cases, under title of *Subacute General Anterior Spinal Paralysis.*

Case V. (Obs. LXXXI.)—Male, aged fifty-five. Gradual palsy of lower limbs, with wasting of muscles; no affection of bladder. In a year, extension of atrophic palsy to upper limbs and trunk; weakness of masticatory muscles, and slow speech. Slight anæsthesia in lower limbs; none elsewhere. Autopsy showed nervous centres healthy to naked eye.

Case VI. (Obs. LXXXII.)—Female, aged ——. Gradually-developed palsy of right arm, both legs, and, lastly, left arm; speech and deglutition difficult. When seen, loss of electro-muscular contractility in many muscles in trunk and limbs; much wasting of muscles of limbs. Sensibility and bladder functions unaffected.

Case VII. (Obs. LXXXIII.)—Male, aged fifty-one. Gradually-developed palsy (descending) of all limbs, with numbness and with slight anæsthesia, but no bladder trouble. Spontaneous partial recovery thirteen months later. When seen, five years later, presents atrophy and palsy of many muscles of forearms and hands.

Cases VIII. and IX. By Moritz Meyer.—" The two Barons von H., twin-brothers, well-built, fine, large men, uniformly healthy, in their eighteenth year, simultaneously fell sick with the measles. These, having run an apparently favorable course, were followed, in both, with a paralysis of the legs, inducing a constantly-increasing emaciation of those

parts." He then states that he saw them six years later, and minutely describes the wasting of many muscles of the lower limbs, and the consequent deformities. He adds, "The sensibility of the skin and muscles was perfectly preserved." There was reduction of electro-muscular contractility in the wasted muscles.

Case X.—Dr. W. A. Hammond,[1] under the head of progressive muscular atrophy, relates the following case, which I take to have been one of subacute spinal paralysis: An adult male had a first illness, characterized by electric pains in the legs, weakness, and head-symptoms (confusion of ideas, vertigo, dim vision, headache, etc.). There was also numbness of both upper and lower extremities. He partially recovered; but in May, 1867, there was a return of the head-symptoms, the electric pains, and numbness; to which were superadded cramps, fibrillary contractions in both hands and legs, with tingling and twitching. In the course of three weeks he was obliged to use crutches. From this time he noticed the atrophy of the muscles of both legs, and it has gradually extended till it has involved the muscles of the lower third of both thighs. In the legs, the gastrocnemii and solei and the extensors are almost destroyed. Condition of sensibility not stated.

Charcot's Cases; first published in "*Considérations sur l'atrophie aiguë des cellules motrices.*" Par Alfred Petitfils. Paris, 1873, p. 72, *et seq.*

Case XI.—Male, aged twenty-seven (seen by M. Charcot in 1863). In 1859 paralysis appeared, preceded by pain in left side; weakness of left lower limb, without numbness; in twenty-four hours complete paraplegia and anæsthesia. No numbness or spasm; no affection of sphincter ani or bladder. In next month wasting of some palsied muscles, and lowering of temperature in parts. Later, some patches of anæsthesia, in legs, sides, right axilla. In three months great improve-

[1] *A Treatise on Diseases of the Nervous System.* New York, 1871, p. 666.

ment (moxas to spine, KI. internally). When examined showed atrophy of left natis, leg, and foot; of left lower abdominal muscles; of right thigh, anterior part. Atrophied muscles have lost electro-muscular contractility. Skin over wasted muscles colder than elsewhere. Charcot recognized similarity to infantile palsy.

CASE XII.—Male (seen by Charcot in 1871). In February, 1871, severe dysentery, followed in eight days by weakness of upper extremities, and complete palsy of the lower. Palsy, with complete flaccidity of muscles. After a few days lower limbs were cold. The arms recovered soon; and in eight days imperfect walking was possible. Since, progressive improvement. When seen, no anæsthesia existed; the anterior parts of thighs are wasted and flabby, and have lost electro-muscular reaction. Never any sphincter ani or bladder palsy.

CASE XIII.—Male, aged thirty-five (seen by Charcot, October, 1871). In the month of August preceding, this man suffered from *malaise* for four days; on fifth day, right arm feeble and tremulous. Later, on same day, right arm paralyzed; then left leg, right leg, and left arm. Accompanying fever, with delirium, lasting one week. At consultation, there is noted wasting of right arm and left leg; no anæsthesia; formication from time to time. Never any rectal or vesical symptoms. No bed-sore.

CASE XIV.—By Prof. Cuming, of Belfast.[1] Male, aged forty. Exposure to cold; numbness in hands; next day walking difficult. On fourth day, complete palsy of all limbs, without anæsthesia. Later, spasms, and lancinating pains in lower limbs. Gradual return of voluntary movements in three months. Cure, with *main-en-griffe.* No bed-sore; no vesical or rectal sphincter palsy.

CASE XV. By Gombault.—Female, aged sixty-seven. On 1st of January, 1867 (sixty-two years of age), in one day experienced great numbness, followed by palsy of all limbs. Sensibility normal; respiration, deglutition, and brain-functions not impaired; no bed-sore, no palsy of bladder or of sphincter

[1] *Dublin Quarterly Journal of Medicine*, 1869, p. 471.

ani. Pain in back at beginning, and for months afterward. Was brought to hospital in fifteen days; had sensibility, but was completely palsied. No contracture of palsied limbs. After two years began to improve gradually; and in three and a half years was able to walk a little with help of a stick. Upper limbs improved first. Examination in 1872 shows slight wasting of arms and forearms, much atrophy of hand-muscles, thenar eminences and interossei: *main-en-griffe.* Extensors of hands very weak. Muscles of arm and forearm show fibrillary contractions. Much atrophy of many muscles of legs. Loss of electro-muscular reaction in hands and extensor group of forearm; lessened reaction in whole of arm and forearm. Reaction diminished in muscles of lower limbs. No numbness, or anæsthesia, or pain. Death, July 19, 1872.

Case XVI. By M. Bernhardt.—Male, aged thirty-five. Exposure to cold while perspiring, after exhausting diarrhœal discharges. In the course of forty-eight to seventy-two hours an extensive paralysis was developed, affecting the muscles of the extremities, unaccompanied by spasms, or by cerebral symptoms. There was no fever. In a few days the palsy of the extremities became absolute, and so remained for months. The affected muscles rapidly lost their electro-muscular excitability, at least to the induced current, and much muscular atrophy ensued. Yet sensibility remained undiminished, the bladder and sphincter ani were not paralyzed, no bed-sore appeared. The respiratory muscles escaped. Reflex movements difficult to excite. Spine not tender; some spontaneous pain in back. Some return of movement in fingers in about three weeks, and afterward progressive improvement. Patient able to walk alone only after eleven months. At an early stage pains occurred in all the limbs.

Bernhardt refers to two other cases brought to his notice by Profs. Traube and Westphal; but the details given are insufficient to justify their acceptance.

Case XVII. (Personal).—Male, aged twenty-four. Seen February 16, 1871. In early life was a healthy and temperate boy; never had syphilis. In 1865 was run away with on horse-

back, striking a tree with his left arm and side of head. Fell from his horse and remained unconscious several minutes. Had no subsequent head-symptoms. In August of the same year, after having been repeatedly wet through while at work upon his farm, he noticed that his legs tingled and felt numb, and that his feet were heavy, so that he easily stumbled. In the course of three months the difficulty in walking was so great that he took to his bed. At that time he had no constriction around body, no loss of feeling, no bladder or rectal sphincter palsy, no spasmodic movements. All the limbs tingled and felt numb and were absolutely paralyzed. He had pain in the back, in its lower part, and between the shoulders. Head was always clear, and the special senses normal. Took strychnine for a time without any effect, good or bad. In the spring of 1866 (?) began to sit up in bed and in a chair. In summer of same year walked with help, and has since progressively improved. No bed-sore at any time. Examination shows nothing abnormal in any part above legs. The thighs are strong; the legs are swung as wholes, the feet not moving in flexion. This is owing to extreme atrophy of the anterior tibial group of muscles. The extensors of feet act fairly, and are but little below their normal size. The plantar muscles are evidently atrophied. No deformity exists. Sensibility in all its modes is perfect in lower extremities. The anterior tibial muscles do not respond to strong faradic current, though it is "felt down to the bone." There is some sense of weight in lower part of back, and slight tenderness over lower lumbar vertebræ. At one time the sexual power was abolished; is now normal. This patient returned to his home in North Carolina, not being heard of since.

My notes indicate that I looked upon the case as one of congestion of the spinal cord.

Case XVIII. (Personal).—Female, unmarried, aged twenty years. Admitted to the Epileptic and Paralytic Hospital, Blackwell's Island, service of Dr. E. C. Seguin, November, 1871. Patient presents a paralyzed and extremely atrophied left leg, and gives the following imperfect history: The trouble

began nine months ago, suddenly during sleep, with painful contractions: she then gradually (?) lost power in the left leg: no other limb affected. The patient cannot state how long a time elapsed between the first symptom and the discovery of palsy. She adds that, on the day before the attack, her left leg felt quite cold and a little numb; and that her menses were suppressed. No cause is apparent—no hereditary influence, no injury.

Examination: Left foot is drawn up in moderate *pes equinus*, with inward inclination. No voluntary movements below the knee. The patient's answers to the æsthesiometer test are unreliable; sensibility to painful impressions is somewhat impaired, that to temperature preserved; tickling is felt equally on both feet. Pressure shows tenderness over the lumbar vertebræ; no spontaneous pain. The right calf measures 26.9 c. in circumference, the left 23.7 c. There is absolute loss of electro-muscular contractility in all the muscles of left leg. The limb is very cold and its circulation feeble. I frequently called the attention of the resident staff and of friends to this remarkable case, as one of the same kind as that which, occurring in the early years of life, we call infantile spinal palsy.

The subsequent history need not be reported. No treatment did any good; the girl remained in the hospital without any active symptom, and went away October 3, 1873, carrying this wasted left leg. She was employed as a help in the wards of the Convalescent Hospital on Hart's Island, and was there much exposed to cold.

The second attack, of which patient gives a good account, came on late in December, 1873. Had pains "like rheumatism" in right leg; there was a feeling of pins and needles in the limb, this numbness extending above the knee. She is positive that on the fourth day the right leg was completely paralyzed. No symptoms in left leg. No bed-sore, and no affection of bladder or rectum. Re-admitted to the Epileptic and Paralytic Hospital, March 3, 1874, with atrophy and palsy of both legs; no acute symptoms.

During the spring and summer this patient rather gradually lost strength in the thighs, in the right most. She also exhibited a variety of interesting visceral disturbances, consisting of amenorrhœa, lasting two and three months; the menses then appearing with much pain, the blood abundant and in clots; there were also pains in the back and lower abdomen. On many days in this period the urine had to be drawn off with the catheter, and it often was bloody, exhibiting a heavy mucous deposit, and containing albumen. The microscope showed only leucocytes and a variety of epithelial cells—there being probably both pyelitis and cystitis. Since the middle of September has not required the catheter, and, with exception of palsy, has been better.

Re-examined October 25, 1874. Patient, when she first came in this year, walked ill with a crutch and stick; is now able to walk with two sticks (result of education). Cannot stand or walk without help. The patient is a stout and healthy girl, exhibiting nothing abnormal above the hips. Both lower extremities are extensively palsied and much wasted. The left leg (first attacked in 1871) shows no voluntary movement below the knee, with exception of slight separation of the toes. As the patient lies on the bed she is able to raise the extended limb as a whole; but the strength at knee-joint is small. The thigh is thin and flabby; the leg is the seat of extreme atrophy, and looks just like the same part in cases of infantile spinal palsy, there being apparently only connective tissue and fat around the bones, the skin being bluish and very cold to the touch. The right lower extremity (paralyzed in 1873) is in a very similar though less extreme state. All voluntary movements are possible with the foot, though they are feebly performed. The limb, as a whole, cannot be raised from the bed, and flexion at knee-joint is weak. The quadriceps extensor femoris is wholly paralyzed; the flexors of the thigh upon the body act feebly; the adductors fairly. Both feet lie extended and adducted; toes flexed. The right leg is, like the left, extremely wasted, bluish, and quite cold. Sensibility to contact, pain, and temperature,

is preserved in both limbs. Tickling is felt, but produces no reflex movement in the palsied parts. The electro-muscular reaction of the atrophied muscles of both limbs is lost (both currents). At present, urine is passed normally. The patient's arms, shoulders, and chest, are large and rounded, standing in remarkable contrast to the dwindled legs. There have been no bed-sores and no spinal epilepsy.

Circumference of	right thigh (lower third)....	31.5 c.
"	left " " "	30.5
"	right calf	24.0
"	left "	21.5
"	forearms..................	25.0

On a healthy girl (non-palsied) of same proportions as the patient, the following measurements are obtained:

Circumference of	right calf.................	35.0 c.
"	left "	34.5
"	forearms.................	24.0

The patient having been in bed some time, well covered up, has a thermometer held between the great and second toes of each foot for three minutes, with results: Right side, 84.25° Fahr.; left side, 86° Fahr.

Case XIX. (Personal). Male, aged about forty, American, a bar-tender by occupation. Seen at South Shaftsbury, Vermont, with Drs. Rogers and Morgan, January 3, 1874.

In September, 1873, caught a severe "cold," had pain in bones, obstinate constipation, excessive vomiting, also severe cough with much expectoration. Relief was obtained by purging, after symptoms had lasted three weeks. On the second day of this attack he had suppression of urine; on the third day his face and feet were swollen; the swelling in face lasted three to four weeks. At the end of first week he experienced coldness in legs as high as the knees; not beginning in any small part, most marked in calves. This coldness was objective as well as subjective, and lasted ten days, during which time there was no numbness, and only a doubtful stiffness of feet; was up each day. Relieved by strychnia; about October 16th was again at work standing in a damp and

cold bar-room. He had a feeling as if feet were "clumpy" (heavy ?). Had slight numbness in thumb, index, and medius of both hands, enough to prevent writing. Late in the month feet again became cold and heavy, and in the course of two days the vomiting returned, lasting four days. The physician who treated him, in Troy, said that he had not Bright's disease. Patient returned to South Shaftsbury, about October 23d; the feet were then weaker than ever, and he could barely walk up a slight hill. After the 24th, the vomiting moderated, and he improved generally. His feet were cold, stiff, but not numb. His brother-in-law, Dr. Rogers, thinks that patient then had a degree of anæsthesia in feet; he could still walk without a stick. The legs rapidly became weaker, so that in two weeks he could barely stand. November 6th, patient ceased going down-stairs, and a week later, legs were insensible to all impressions but severe pain; limbs lost in bed. There was complete palsy of parts below ankle (leg-muscles paralyzed); thigh-movements good; no reflex movements, or contracture; no bladder or rectum disorder. Suffered much from a painful throbbing in soles of feet, most in right foot. Had slight fever; about November 15th muscles of legs and feet began to waste. During the second week of November the median distribution in fingers became anæsthetic; he could use fingers supplied by ulnar nerve. The hands and forearms wasted rapidly. There was emaciation (almost wasting) of thighs and arms. December 8th, a degree of sensibility was discovered in feet, and in the fingers a week earlier. Since, there had been gradual improvement in voluntary movement and sensibility. The wasting has, however, continued to increase until now. On December 21st, motion was observed in toes. Has had no bed-sore; no sensation of constriction about the waist or elsewhere. In the last few days has had consciousness of location of all parts of lower limbs except toes. Throughout, the right side has been the more affected. Has had no head-symptoms.

Examination: Nothing objective about head. The movements at shoulders and elbows are good. Patient grasps three

kilogrammes with right hand, five kilogrammes with the left. The fingers appear normal, except that the second and third phalanges of forefingers cannot be extended. Opposes thumb fairly well, though he cannot make O with thumb and index. Interossei, though much wasted, still act. Thenar muscles (especially the opponens) are much atrophied. No deformity while at rest. Co-ordinates fairly well. Æsthesiometer shows anæsthesia; the points being distinguished at distances of from eight to fourteen mm. on tips of fingers in median distribution, and at five to eight mm. on the parts supplied by the ulnar nerve.

The toes are in forced flexion. As he lies, he can raise legs high up, and bend knees well; can move all joints except those of great-toes; though the movement of the other toes is hardly perceptible. There is only slight wasting of thighs; but the legs are much shrunken, especially in front. The right calf measures 25.25 c. in circumference, the left calf 24.5 c. The skin below ankles is decidedly anæsthetic, slight contact being hardly perceived; the feet and legs are hyperalgesic, there being at the same place a retardation in the perception of pain of from five to eighteen seconds. The temperature of the legs was warm until two weeks ago, since which time they have been cooling. After first attack of vomiting had no sweating except a very little about head, until sensibility began to return nearly four weeks ago, when perspiration appeared everywhere. During illness, absence of erection, until very lately. Throbbing pain in balls of feet has nearly ceased. The muscles of legs respond faintly to faradism, with exception of extensor proprius pollicis, which does not contract; all the hand-muscles respond, those of the thenar eminences poorly. With galvanism all these muscles contract under twelve or twenty elements, current interrupted. The left leg (less palsied) shows contraction with less faradic and more galvanic current; while the opposite is true of the more palsied right leg. The optic disks are a little hazy, especially the left. The patient never had syphilis, was never injured, has never committed excesses in alcohol, tobacco, or

with women. His occupation of bar-tender obliged him to stand in a damp basement from 5 A. M. to midnight. The urine is normal. Diagnosis: inflammation of the anterior horns of the spinal cord. Advise tonics, galvanic current now to muscles, to be followed by faradism in a few weeks.

On February 28th, Dr. Rogers wrote me that "he has steadily improved since the time you saw him. He is now able to walk about his room by sliding a chair before him. Can raise himself from his chair by taking hold of a chair with hands. His legs about the calves are now an inch and a half larger than when you were here. Sensation is, I think, much better." I have since learned that this patient wholly recovered.

Case XX. (Personal).—Male, aged forty-seven, American inventor, seen March 23, 1874. A very large, healthy man, who has worked hard at scientific problems. In last two or three years hæmorrhages from lower bowel, and various dysæsthesiæ about the head. These consisted in pain in upper neck and back of head, in a sense of pressure on top of and behind head, etc., without impairment of intellect.

In October, 1873, had a severe cold, which was followed by much coughing. The time of beginning of symptoms of paralytic affection is difficult to determine with accuracy, because numbness and paresis crept upon him so gradually. A few days before November 10th he had complained of a sense of coldness (not objective, according to both patient and wife) in feet and legs. The numbness began in that week, appearing at about same time in ends of upper and lower extremities. On this day he went to Boston on business: he could walk with difficulty with the help of a cane, and the support of his son; he could fully dress himself. On November 13th he returned, having more paresis, and remained at home. At this early date there was swelling of the feet. On November 14th the legs and forearms were quite weak; the legs swollen and glossy; the seat of subjective cold. November 17th, could not leave bed, or sit up in it. There was much formication extending up to the knees, and half-way up forearms. No

head or eye symptoms; no affection of bladder or of rectal sphincter. The feet were anæsthetic (?), though legs were not lost in bed. No reflex movements; no marked wasting of limbs; no fever; the œdema continued, the hands being a little swollen; the urine was "thick." He had a partial constriction band extending over lower ribs of right side, and a sense of tightness just above line of numbness in lower part of thighs. He was dry-cupped and leeched over spine. January, 1874: About middle of this month more intense formication ushered in recovery. After a few days, sensibility returned in part of feet, and patient was able to perform some movements. Toward the middle of February he began to sit up in bed, and in a chair, gaining daily. The numbness receded toward extremity of limbs; feet remained a little swollen. Since that time has gradually improved; he now walks a little with the help of a crutch and stick.

During the course of the illness there were some changes in the intensity of the symptoms from day to day, but not great ones. He had diffused spinal pain; and when at the worst had a localized pain between the shoulders, relieved by cupping. There occurred some degree of wasting in parts below the knees. The subjective coldness continued until the numbness had almost ceased. In second month of illness, and from time to time since, had numbness in distribution of second branch of left fifth nerve on face. The head symptoms, which together with irritability had annoyed him so long, disappeared during illness. Examination: patient's general condition fairly good. Walks with a cane, without dragging or jerking leg; step is that of simple weakness. No facial symptoms. Moves arms in all directions. Movements of hands are good, except extension, which, almost complete for the fingers of the right hand, is far from good on the left side, especially for the thumb and index. Palmar muscles good; no atrophy, only general wasting. Sensibility of hands to contact and pain good. Grasp equals twenty-six kilogrammes with right hand, sixteen kilogrammes with left. A thermometer placed for three minutes in the fold between

thumb and palm shows 96.1° Fahr. in right hand, and 97° Fahr. in left. Co-ordinates well.

Lower extremities: all movements possible except that the left foot is but slightly movable; the left toes can hardly be extended; those of right foot move a little more. The toes are somewhat contracted in flexion. No wasting of any group of muscles in legs; co-ordination good. Sensibility to contact is normal as far as ankle, dull below that point, very dull on toes. Slight retardation of impressions of pain on toes. Toes are seat of numbness. The soles are sensitive. Can stand a moment without cane and with eyes closed. Has no sense of constriction anywhere. Electrical examination shows very great loss of electro-muscular reaction to faradism in palsied muscles; almost complete in left extensor indicis, and in extensors of toes, and flexors of foot on both sides; not much reaction in muscles of calves. These various muscles contract well under the galvanic current.

Advised the use of strychnia in small doses, to be increased; of galvanism and faradism; and of exercise with daily friction and passive movement.

It should be added that, when I examined this patient, there was no œdema of feet, and the urine, although dense, was free from any sign of disease of the kidney.

A letter received from a member of patient's family, on October 24th, speaks of Mr. H. as having almost wholly recovered; walking freely with help of a stick, and carrying on his business.

Case XXI. (Personal).—Male, aged twenty-three, single; American. Seen at Worcester, Mass., with Dr. Francis, June 29, 1874. Was a healthy, strong, and sober youth. In November, 1871, went West, to Cincinnati, where he committed sexual excesses, though without contracting venereal disease. He then went out to Missouri, where he was employed on railways, as division superintendent, clerk, etc., being much exposed to the elements. In midwinter, "about two months after reaching Missouri," patient had an attack, called, by physicians, "choreic palsy;" all his limbs being

paretic, and he walking like a drunken man. He had numbness in all his limbs; there was no affection of rectal sphincter or bladder; he could sit up, and walk with help; was cured of all but numbness and weakness of left hand, by chloride of arsenic (?), in the course of two or three months (this being early summer of 1872). About the end of this period of convalescence, he had a week of sickness, in bed, from malarial intermittent fever.

In September, 1872, with his left hand still slightly numb and weak, he went to Fond du Lac, Wis.; settling there as a storekeeper. He had to travel somewhat, and had much financial responsibility. During the winter strychnia was given him for the cure of the left-hand weakness; but he then grew worse, and the second attack of palsy developed.

He returned East, very much paralyzed in all limbs, in May, 1873, the disease having made rapid progress in the preceding three weeks. When received at home, about the last of May, his right extremities were quite powerless—those on the left side could still be used, he walking, though very imperfectly, when supported. He could sit up on the edge of the bed, or in a chair. He then talked and swallowed well; had no strabismus or impairment of sight. He had the sensation of a band around body at umbilicus (which feeling, afterward, extended higher up). There was much numbness in all extremities; anæsthesia doubtful. There was slight muscular atrophy. No sign of spinal epilepsy (jerking or stiffness of palsied parts). Constipation quite obstinate; no bladder-palsy, though he was forced to empty the viscus quickly after sense of distention appeared. During the summer the patient grew progressively worse; in the autumn, he could not walk with help; and, later on, sitting up in bed became impossible.

About November 27, 1873, there appeared external strabismus of the left eye, and diplopia; also doubtful paresis of facial muscles.

In January, 1874, a degree of spontaneous improvement was observed. The patient was once more able to sit up on the edge of his bed, and could put his hands up to his head,

especially the left; the strabismus disappeared, though the left pupil remained wide.

In February, Mr. B. again lost ground, nearly complete palsy existing; speech was lost quite rapidly (not suddenly); swallowing became difficult, and strabismus reappeared.

June 29th.—In the last two months has lain in bed almost completely paralyzed in limbs, face, eyes, tongue, and throat; the chest-walls and diaphragm escaping. The muscles of the extremities have steadily wasted, and deformities have appeared in the hands and feet. The difficulty of deglutition has been with solids; no regurgitation of fluids through the nose having occurred. Has been anæsthetic (?) to pinching in feet and legs at times. Has had a good deal of spinal epilepsy (tonic and clonic spasms in the palsied parts following any irritation). Has had a sense of tightness about chest and belly. Has of late passed urine and fæces involuntarily, though rarely unconsciously. Much muscular wasting. Has often complained of dim as well as of double vision.

Examination.—Patient lies quite helpless on his back; when he is turned, spinal epilepsy appears in whole body. The possible voluntary movements are slight motion of the left great-toe, and of left fingers. His sisters say that the day before he could raise both hands a little above bed. Eyes are moved imperfectly, and in a quasi-ataxic way, in every direction, except that prohibited by the left third-nerve palsy. There is no ptosis, but the pupil is wide. The ophthalmoscope shows the fundus without marked lesion, though the disks are, perhaps, abnormally white. The face is a mask, though the upper facial muscles act fairly, and patient can purse lips. He cannot whistle, or make any articulate sound —uttering only a low grunt. The tongue can be protruded only to teeth; it lies undeviated in the floor of the mouth, atrophied and much shriveled (folds longitudinal) in anterior half. The intercostal muscles and the diaphragm act well; respirations fairly deep—twenty-four per minute. Patient has escaped bronchitis.

The state of sensibility is difficult to examine, because

patient can make no sign. Pinching is surely felt everywhere. The muscles of the palsied parts are very much wasted; those of the tongue, forearms, legs, face (?), especially. The hands show the deformity known as "*main-en-griffe.*" The thenar eminences are much wasted. In feet, there is partial *pes equinus*, with toes flexed. No ulceration has appeared anywhere. The atrophied limbs are cool (thermometer showing 89° Fahr. in shaded room). All (?) the atrophied muscles respond, though feebly, to the faradic current.

Diagnosis, myelitis, or degeneration of the anterior horns of gray matter of cord; the motor part being involved from the third cerebral nerve downward, with probably recent extension of myelitis, to deeper parts of cord at some points.

October 20th.—A letter from Dr. Francis states that patient is in substantially the same condition as in June; the only change being some dementia.

Case XXII.—In the practice of Dr. T. A. McBride, of this city; seen in consultation October 23, 1874. Male, aged twenty-eight years. Has in the last few years led a very fast life, drinking a great deal, and committing sexual excesses. Some time ago had a soft chancre; never any secondary symptoms. Has beginning pulmonary phthisis. From August 27th to date has had more or less subacute articular rheumatism; knees and ankles most involved.

Ten days ago (October 13th) first noticed numbness in feet, gradually extending up to knees. Has some cramp-like pains in legs. Two days after (21st) observed that the tips of all fingers were numb; those supplied by the median nerve most. First paresis one week ago, three days after first numbness. He noticed that the left great-toe could not be extended; since has had a dragging and staggering gait, and has remained in bed. To-day discovers that the extensor muscles of fingers are weak, the left middle finger dropping much below the others during extension. Has had no pain in back; no vesical or rectal symptom. He complains much of coldness of legs.

Examination shows patient to be a nervous, rather delicate

subject, with so much lung-disease as to give a nearly uniform daily fever of 1.5° Fahr. He lies in bed, but is able to sit up, and can move his legs in every direction, though feebly. The left great-toe and left middle finger cannot be fully extended. Extension of the hands and fingers is incomplete and weak. Flexion of feet upon legs is weakly performed; the strength at knee-joints is reduced. The anterior aspect of both legs and the extensor surface of both forearms are evidently wasted. Tested with the faradic current, the anterior tibial muscles (those of the left side more) show diminished reaction; the left extensor proprius pollicis not responding at all. The extensor muscles on the forearm have also lost much of their excitability, those on the left side especially. Sensibility is not impaired in fingers. In the lower extremities, below the middle of the legs, there is marked anæsthesia to simple contact and to æsthesiometer-test, with some errors in localization of impressions. Pain and temperature are perceived everywhere, though the former kind of impressions is perceived after a retardation of several seconds, and the impressions persist for several seconds. The right calf measures 28 c. in circumference, the left 27.5 c. The patient has "lost his legs" a few times in the last few days. There have been no reflex movements (it is difficult to provoke any now); no affection of the bladder or sphincter ani; no threatening of bedsore; no cerebral symptoms; no weakness of chest and abdominal muscles. The patient's legs are cold, objectively and subjectively. He has had no feeling of constriction around any part of the body. The spine is not tender. I diagnosed subacute spinal paralysis, and advised counter-irritation to the back and the internal administration of Squibb's fluid extract of ergot in free doses.

On November 1st I again saw the patient. He has in some respects improved. The anæsthesia of the lower limbs has decreased, there being almost no retardation of impressions. Some feeling of tight band about knees at times. The legs have lost about .5 c. in circumference, and voluntary movements are as before. Fibrillary contractions are

abundant in the anterior tibial muscles. The upper extremities are worse than ten days ago. The numbness extends up to second joint of fingers, and on the radial side of each index there is much tactile anæsthesia. General condition rather better. No affection of inspiratory or expiratory muscles; no bladder or rectum weakness; no bed-sore. Advised to dry cup the back and continue Squibb's ergot in doses of ℥ss a day, with quinia sulph. gr. v, in the morning; nutritious food, cod-liver oil and stimulants.[1]

Semeiology.—I shall divide my remarks upon the symptoms recorded in the above histories into two categories: 1. I shall study the symptoms individually, or analytically; and, 2. shall attempt to decide whether the symptom-grouping is such as to justify us in admitting the name acute and subacute spinal paralysis into our nomenclature of nervous diseases.

The various symptoms noted in the above cases are naturally divisible into three groups, viz., disorders of motility, disorders of sensibility, and disorders of nutrition.

A. Disorders of Motility.—These have consisted in paresis and akinesis. In some of the Cases (I., II., III., IV., VIII., IX., XI., XII., XIII., XIV., XV., XVI., XVIII.), the loss of voluntary motion appeared well marked in an acute way; that is, within twenty-four or seventy-two hours after the first symptoms. In others (V., VI., VII., X., XVII., XIX., XX., XXI.), the palsy crept gradually upon the patient; in some of these cases so slowly as almost to deserve the designation of chronic (Cases X. and XVII.). In fact, a study of the twenty-two cases will show that every degree in rapidity of development of paralysis may be observed, between an almost sudden onset (Gombault's case, XV.), to a very gradual one (personal observation, XVII.).

[1] On December 9th, I saw this patient with Dr. McBride. Sensibility is perfectly restored, except on radial border of left index-finger, where slight anæsthesia remains. All voluntary movements are now possible. The muscles are firmer; patient has gained flesh; calves measure 29.25 c. This patient was severely dry cupped, and took for a while ℥j Squibb's ext. ergotæ fl. with belladonna. No advance in the pulmonary disease.

The distribution of the paresis and akinesis has been either general or paraplegic. Cases I., II., III., IV., V., VI., VII., XII., XIV., XV., XVI., XVII., XIX., XX., XXI., XXII., were instances of general paralysis, i. e., paralysis of all four extremities. Cases VIII., IX., X., XI., XIII., XVIII., were paraplegiform, i. e., the palsy affected one or both lower extremities. Case XXI. (personal) stands alone as an example of paralysis having involved that part of the motor spinal tract which is intracranial. In Cases V. and VI. the organs of deglutition and speech were partially paralyzed. The akinesis, as a rule, affected whole groups of muscles, and involved parts which are symmetrical and homologous. That is to say, the anterior muscles of the legs and the extensor group of the forearms were paralyzed together in several cases. Except in two or three instances the palsy appears to have been almost equal on each side of the body.

A striking feature to be noticed in nearly all the histories is the retrocession of paralysis, and the spontaneous return of voluntary movements in certain parts.

A remarkable immunity to palsy is shown by certain muscles; those of the neck, back, chest, abdomen, and the sphincter ani. The bladder was not paralyzed in any case (possible exception in Case XXI. and in Case XVIII., where temporary retention occurred). This limitation of the akinesis to muscles concerned in the acts of relation will prove a most important element in diagnosis.

Hyperkinesis, or spasm, was absent in almost every case. Late in the course of Case XXI., spinal epilepsy (tonico-clonic spasm of palsied parts) showed itself, and in Cases XXII. (actually under observation) X., and XV., fibrillary contractions were observed in the palsied muscles.

B. Disorders of Sensibility.—The general statement that sensibility was not permanently impaired in these cases is, I think, fair. In more than half of them no anæsthesia existed at any time (I., II., III., IV., VI., VIII., IX., XII., XIII., XIV., XV., XVI., XVII., XVIII.); in the remaining seven cases a moderate degree of loss of tactile sensibility existed, a

temporary defect in most of them. It was only in Cases XI. and XIX. that total anæsthesia of a limb, or large part of a limb, was noted; and that was temporary.

Hyperæsthesia of the palsied parts in the shape of tenderness to pressure was noted only in Cases XX. and XXII.

Dysæsthesiæ, or morbid sensations, were abundant; consisting in prickings and numbness in Cases I., II., IV., VI., X., XIII., XIV., XV., XVII., XVIII., XIX., XX., XXI. This numbness was in nearly every instance an early symptom, in many cases the earliest symptom. Another variety of dysæsthesia experienced by five patients early in the course of the disease was subjective cold in the palsied (or to be palsied) parts (Cases XVIII., XIX., XX., XXII.). The morbid sensation called constricting-band feeling, or cincture feeling, was observed only in Cases XX., XXI., and XXII., over the lower part of the thighs in the first case, around the waist (late in the disease) in the second, and over knees in the third. Pain in the limbs to be paralyzed was present at the beginning of the disease in Cases I., II., IV., X., XIV., XVI., XVII., XVIII., XIX., XXII. This pain was sometimes severe, "cramp-like" in the language of one patient; in another patient, it was a tearing pain in the soles of the feet; in another (X.) it was an "electric" pain. Spontaneous pain in the back was noted in five cases (I., II., IV., XVII., XX.), but was great only in the first. No marked tenderness of spinal region was observed in any case.

C. Disorders of Nutrition were only of one order, viz., muscular atrophy. This atrophy was developed more or less rapidly and extensively in different cases, but was recognizable in all. It affected whole muscles, or whole groups of muscles, differing in this respect from the irregular wasting called progressive muscular atrophy. In Case XXI. the tongue was much atrophied, and the facial muscles slightly. Fibrillary contractions were seen to accompany this wasting in Cases X., XV., and XXII.

Together with muscular atrophy, there occurred loss of

electro-muscular reaction to the electrical (faradic) current. This was observed very early in several cases, and was well marked even where the atrophy was but slight (Cases XX. and XXII.). In Case XVIII. the atrophied muscles did not contract even under strong galvanic interruptions. In all my own cases (XVII., XVIII., XIX., XX., XXI., XXII.) the paralyzed and wasted parts were much colder (objectively) than the healthy parts. The reduction in temperature was not as great as in the cases of infantile spinal paralysis. In the case represented in the photograph the lower limbs are very cold to the touch, and look purplish. No bed-sore occurred in any case, nor were lesions of the skin,[1] nails, or hair, noted.

Visceral complications were observed in some patients, and these may have been nutritive disorders secondary to the spinal disease. In Case XVIII., cystitis, pyelitis, and amenorrhœa, were at one time present; in Case XIX., vomiting, bronchitis, suppression of urine, and swelling of the feet without Bright's disease, were noted; in Case XX. there was œdema of the feet, legs, and hands, without albuminuria.

Fever was recorded as present at the onset in Cases I., III., IV., XIII., XIX. Cases VIII. and IX. issued out of rubeola.

From the above analysis it appears that the symptoms of spinal paralysis of the adult (acute and subacute) are remarkably constant, and we may safely give the following grouping as characteristic: Dysæsthesiæ and slight temporary anæsthesia, paresis and akinesis, both these symptoms affecting the extremities, and in rare cases the face, eyes, tongue, and throat; not affecting the respiratory muscles, nor those of the back and abdomen, nor the bladder, nor the sphincter ani. Muscular atrophy in the paralyzed parts. Loss of electro-muscular contractility (to faradic current) in

[1] In Case XXII., after my second visit (third week of palsy) there appeared marked vaso-motor paresis, as shown by the occurrence of red patches wherever pressure was made, on legs, knees, and malleoli, arms, and back; the redness lasting some time.

the atrophied muscles. A strong tendency to spontaneous retrocession of the palsy, and to spontaneous cure.

The important negative characters of this affection are: Absence of palsy of bladder, or of sphincter ani, or of respiratory muscles. No bed-sores. No great and extensive anæsthesia. No spinal epilepsy.

The course of the affection may be acute, subacute, or chronic; the majority of the recorded cases being of the first variety. In spite of this element of time, the symptom-grouping remains remarkably uniform in all varieties. In one of my own cases (XVIII.) the left leg was gradually paralyzed in 1871, and in 1873 the right leg was powerless after an illness of four days, and probably was so earlier. In Case XV. (Gombault's) the general paralysis was fully developed in one day. In my own case (XVI.) the patient's legs became weak slowly during three months' time.

Diagnosis.—These remarks upon the symptoms and course of spinal paralysis of the adult naturally lead to the consideration of its diagnosis, both positive and differential.

The positive diagnosis is to be made, and that in nearly all cases with ease, from the presence of the following symptoms, arranged in order of importance: Paresis or akinesis, affecting the muscles of relational life; extremities, tongue, face, or even eyes. Atrophy of, and loss of electro-muscular contractility (faradic) in the palsied muscles. Also from the absence of extensive or permanent anæsthesia, spinal epilepsy, bed-sores, bladder or sphincter ani palsy.

The problem in differential diagnosis will be different in cases in which the above capital symptoms appear in an (*a*) acute, (*b*) subacute, and (*c*) chronic way.

(*a.*) *Acute Spinal Paralysis.*—This resembles infantile spinal paralysis in the most wonderful way; the symptoms of general systematic disturbance being much more marked in the young child (fever, delirium, convulsions). In each class of subjects the akinesis is developed in one or three days, it bears the same characters (with early loss of electro-muscular reaction), and affects the same muscles. Five adult cases

had fever at the beginning (I., III.(?), IV., XII., XVIII.). When speaking of pathological anatomy I expect to convince you that the diseases are identical. The acute spinal palsy of adults, when developed in one day, may bear a resemblance to hæmatomyelia (hæmorrhage into the spinal cord), to softening of the spinal cord, or to central myelitis (localized or diffused). From hæmatomyelia the diagnosis is to be made by the absence of great anæsthesia, and the escape of the bladder and sphincter ani from palsy. Besides, hæmatomyelia produces symptoms almost or quite *suddenly*, whereas the symptoms of the acute form of myelitis we are studying appear in a *rapid* way. Softening of the spinal cord and central myelitis (with limited or diffused lesion) give us anæsthesia, bladder-palsy, spasms in the paralyzed parts (spinal epilepsy) even at a quite early stage. In both these affections it is seldom that, as is the rule in spinal paralysis, the paralysis involves all the limbs in a short space of time; and when the palsy is thus general, in all three of these affections, the intercostal and the abdominal muscles are paralyzed.

(*b.*) *Subacute Spinal Paralysis.*—For a few days the physician may well be in doubt as to whether he has to do with the above disease, or with spinal congestion. In both affections there is peripheral numbness, with gradually-developing palsy; no anæsthesia, no tendency to bed-sore, no palsy of the bladder, or of sphincter ani. In congestion the respiratory muscles do not escape as completely as in spinal palsy. In a few days, however, an important element in diagnosis appears in the shape of loss of electro-muscular contractility (to faradism) in the weakest muscles of the patient with spinal paralysis. The atrophy soon following makes the differential diagnosis sure. There is an affection running its course in ten to twenty days, characterized by symptoms almost identical with those of subacute spinal palsy. There is an akinesis, without much anæsthesia, first appearing in the feet and legs, then ascending and involving the entire trunk and limbs, producing in nearly all cases death by asphyxia. It is upon this palsy of the respiratory muscles that the diagnosis of this

most fatal disease, acute ascending paralysis, is to be made from spinal paralysis. In some of the more recently-observed cases of ascending paralysis, the muscles were found to have lost their electro-muscular reaction at a very early day. At any rate, the relationship between the two diseases is very close; and of this I shall give some additional proof in the section on pathological anatomy. I should add that acute ascending paralysis is sometimes better named acute *descending* paralysis, the palsy first appearing in the arms.[1] The absence of anæsthesia, of bed-sores, of vesical and rectal palsy, of spasmodic movements in the paralyzed parts, will serve to distinguish spinal paralysis from subacute localized myelitis, and from the effects of tumors upon the spinal cord. In the last-named forms of paralysis the muscular irritability is retained or exaggerated.

Cases of subacute spinal paralysis, which develop so slowly as to merit the designation chronic (as Case XVI.), resemble in many ways progressive muscular atrophy: the appearance of the patient may be very deceptive indeed. The patient, whose photograph I have passed around, seems to have progressive muscular atrophy limited to the lower limbs. In true progressive muscular atrophy there are no paralytic symptoms in the strict sense of the word; the loss of power coincides with the wasting of the muscular substance; in spinal paralysis, weakness, even in the most chronic cases, is more prominent than atrophy. In progressive muscular atrophy the wasting affects portions of muscles, and never muscular groups, as in spinal palsy. The electro-muscular contractility is preserved in the muscles which are the seat of disorganization in progressive muscular atrophy, as long as any healthy muscular fibre remains; we see one half of a muscle responding to the faradic current, while the other half shows no reaction. In spinal paralysis the muscles lose their reaction to faradism in groups, and do so before much wasting is apparent. Again, progressive muscular atrophy is strangely apt to strike homologous parts: the arms and

[1] Duchenne, *op. cit.*, p. 446, note.

thighs wasting simultaneously, or the legs and forearms, or the upper thigh and hip at the same time as the shoulders. Fibrillary contractions are very often present in the wasting muscles of patients with progressive muscular atrophy; rarely in chronic and subacute spinal paralysis. Lastly, the course of true progressive muscular atrophy is very much more chronic than that of any form of spinal paralysis.

Pathological Anatomy.—The first thorough autopsy with microscopical examination of the spinal cord, in a case of spinal paralysis of the adult (acute form), was made by Gombault, in 1872, and published early in 1873.[1] If this single autopsy were made in a case belonging to a class of disease with less well-defined and constant symptoms than spinal paralysis, I should feel hesitation in attaching much importance to it. In view of the non-existence of this objection, and of the strong argument by analogy, which may be made out of abundant material collected by the best observers, I feel bound to ask you to believe, with me, that in this autopsy we have a basis for the determination of the pathological nature of the affection we have been studying under the semeiological denomination of spinal paralysis of the adult.

The autopsy of M. Gombault's patient revealed no lesion visible to the naked eye in the brain or spinal cord.

Histological Study.—The atrophied muscles were found to be in the various stages of granulo-fatty degeneration.

The spinal cord was examined after hardening in chromic acid, sections cut from the cord being prepared by Clarke's method. Throughout the whole length of the organ the white columns and the posterior gray horns were found normal; in other words, the lesion was found restricted to the ganglion cells of the anterior horns. It is worth while to quote Gombault's description of the lesion, *verbatim:*

" As regards the alteration of the ganglion cells, it exhibits the same characters as are met with in the progressive atrophy of these cells. Although the cell-degeneration is everywhere well marked, it is, nevertheless, possible to follow

[1] *Archives de physiologie normale et pathologique*, 1873, pp. 80–87.

it in its various stages of development in one section. Close to cells which seem quite normal, others are seen containing a small amount of yellow pigment. In other cells this is so abundant as to surround the nucleus and nucleolus, though these structures are still visible. At this stage the cells tend to assume a globular shape. In a still more advanced degree of degeneration, the cell-processes are shriveled, or are even absent; the nucleus disappears, and the only thing left of the cell is a small, rounded body filled with yellow granules, and surrounded by a thickish envelope, which is stained by the carmine. In some cells which still retain processes, these latter structures may be traced as continuous with the stained envelope. The alteration is diffused, it has affected cells here and there, and a number of these bodies must have disappeared, since in some sections from the cervical region it is not possible to count more than fifteen or twenty cells. The external posterior group, in the cervical and lumbar enlargements, seems to have been attacked by preference. Throughout the whole anterior gray matter there are altered cells. The lowest part of the cervical region seems to have suffered the most. The cells which do not exhibit yellow pigmentation appear to bear some trace of the lesion which must have affected them at some anterior period. They have undergone, for the most part, a reduction in size; and there are very few measuring 0.066 mm.—a size below the average for the ganglion cells of this region."

The medulla oblongata was found to be healthy, except that a few cells of the hypoglossal nucleus were granular. Some anterior roots were in part atrophied. Sections of the nerve-trunks of arm showed some small patches of sclerosis—the majority of the tubules and bundles of nerves being healthy.

In brief, the lesion found in the apparently normal spinal cord of a patient having had typical acute spinal paralysis, was GRANULAR DEGENERATION OF THE GANGLION-CELLS OF THE ANTERIOR HORNS.

What is the lesion present in the nervous centres of pa-

tients dying while the subjects of muscular atrophy? Such is the line of inquiry to be pursued in order to test the value of the autopsy above related.

(*a.*) The disease accompanied by muscular atrophy and loss of electro-muscular reaction, which most resembles the spinal paralysis of adults, is that called *infantile spinal palsy.* For many years this was thought to be a congestive disease, or a myopathic disease, or a form of reflex paralysis. We owe to Charcot,[1] of Paris, and his pupils, the demonstration of the presence of a constant lesion in these cases; a lesion consisting *always* in granular degeneration of the ganglion cells of the anterior horns, and nearly always also in sclerosis (secondary) of the antero-lateral columns of the cord.

Since Prévost published his first autopsy in 1865,[2] every autopsy has yielded the same results in the hands of skilled observers and microscopists of France, Germany, and England.

(*b.*) *Progressive Muscular Atrophy.*—We owe, among many debts, our knowledge of the pathological anatomy of this disease to J. Lockhart Clarke, of London, who, in 1862[3] and subsequent years, published the results of most carefully-made microscopical examinations of the spinal cord in cases of this affection. Hayem, Charcot and Joffroy, and others, have obtained the same results, viz., showing that the essential central lesion of progressive muscular atrophy consists in granular degeneration of the ganglion cells of the anterior horns of the spinal cord in the parts giving origin to the nerves going to the wasted muscles.

(*c.*) Again, in *labio-glosso-laryngeal paralysis*, a disease in which wasting of certain facial and intrabuccal muscles goes hand in hand with their paralysis, Charcot[4] has discovered granular degeneration of the ganglion cells, forming the nu-

[1] See Infantile Spinal Paralysis: a clinical lecture, by Dr. E. C. Seguin. *The Medical Record* (New York), 1874, p. 25.

[2] Prévost, *Comptes-Rendus de la Société de Biologie*, 1865, p. 215.

[3] *British and Foreign Medico-Chirurgical Review*, 1862, ii., 215.

[4] Charcot, in *Archives de physiologie normale et pathologique*, 1870, pp. 247–260.

clei of origin of the hypo-glossal and facial nerves, in the floor of the medulla oblongata.

(*d.*) *Muscular Atrophy occurring as a complication in the course of various diseases of the central nervous system.*—It is a well-known fact that, in hemiplegia caused by a cerebral lesion, the muscles of the paralyzed side retain their irritability in a remarkable way; that they in fact usually respond to electric stimulation more readily than the muscles of the opposite healthy side (Marshall Hall's law). In exceedingly rare instances the paralyzed muscles have been known to lose their electro-muscular excitability, and to undergo rapid atrophy. M. Charcot[1] has had the opportunity of examining the spinal cord in a case of this kind, and he discovered that there was a sclerosis, with degeneration of ganglion-cells, of the anterior horn on the same side as the paralysis, this morbid process being an extension of the descending degeneration, which is usually limited to the lateral white columns. Once in a while we see muscular atrophy complicating progressive locomotor ataxia. Pierret,[2] in 1870, examined the spinal cord of an ataxic patient, having wasted muscles, under Charcot's guidance, and discovered that, in a part of the cervical enlargement corresponding to the wasted arm, the sclerosis had extended from the posterior columns of the spinal cord into the anterior horn, destroying the group of ganglion cells known as the external lateral group. Lastly, in the complex affection, which results in disorganization of the central part of the cord, with formation of a plug or distended cavity, the anterior gray matter is encroached upon, its ganglion cells undergo granular degeneration and disappear—the externally apparent symptom of this lesion being muscular atrophy, which goes, in these cases, along with

[1] Cited by H. Charlton Bastian, Clinical Lectures on the Common Forms of Paralysis from Brain-Disease. Lecture V., Part I.—*The Lancet*, 1874, p. 406.

[2] Pierret, "Sur les altérations de la substance grise de le moelle épinière dans l'ataxie locomotrice, considerées dans leurs rapports avec l'atrophie musculaire qui complique quelquefois cette affection."—*Archives de physiologie normale et pathologique*, 1870, pp. 599–617.

anæsthesia, akinesis, bed-sores, etc.[1] It appears to me that, in the several newly-discovered facts enumerated, viz., that the only lesion always present in infantile spinal paralysis, progressive muscular atrophy, labio-glosso-laryngeal palsy, and in various forms of "symptomatic atrophy," is a granular, pigmentary degeneration of ganglion cells of the anterior horns of the spinal cord, there is a justification of the generalization first made by Charcot. This most able observer and teacher announced, in a clinical lecture delivered at the Salpêtrière in June, 1868, that the relation between granular degeneration of motor nerve cells and muscular atrophy was that of cause and effect. This position Prof. Charcot has since maintained, accumulating fresh evidence year by year in its support. For my own part, I accept this view without hesitation. If we now confront this generalization that granular degeneration of motor nerve-cells produces muscular atrophy, with the bare fact revealed (p. 31) by Gombault's autopsy, that the only lesion in his case of acute spinal palsy was granular, pigmentary degeneration of some of the ganglion cells of the anterior horns of the spinal cord, we cannot avoid concluding that the spinal paralysis of adults, in any one of its forms, depends upon degeneration of anterior ganglion cells.

Whether this degeneration of the ganglion cells is the *only* lesion in spinal paralysis is a question to be solved only by future autopsies. It is well to remember that, in nearly every case of infantile spinal paralysis recently examined, a degree of myelitis (of gray and white matters) coexisted with the cell-degeneration. With respect to the great question, whether the change present in Gombault's case, pigmentary granular degeneration of ganglion cells, is an inflammatory or a degenerative (ischæmic) process: the elements for discussing this complicated problem are not yet in our hands; many autopsies, at different stages (or age) of the lesion, as shown in vari-

[1] See Schüppel, Ueber Hydromyelus, *Archiv der Heilkunde*, 1865, p. 289. Hallopean, Étude sur les myélites chroniques diffuses, *Archives générales de médecine*, 1871, ii., pp. 277, 435, 565; 1872, i., pp. 60, 191.

ous clinical forms, will have to be made. I think it best to assume, as a temporary hypothesis, that there is parenchymatous inflammation in the spinal paralysis of adults (and of children), the results appearing chiefly or only in mal-nutrition and degeneration of the affected ganglion cells.

Consequently I have chosen the pathological name of inflammation of the motor tract of the spinal cord. I prefer it to anterior myelitis, because when the disease ascends above the decussation of the pyramids, it affects parts which, while constituting a part of the motor tract, lie posteriorly. The term motor (or kinesodic) tract I prefer to that of anterior horns of the spinal cord, because it applies to the upper part of the spinal axis, that part which lies in the parts called medulla oblongata, pons Varolii, and crura cerebri by anatomists. The true spinal cord, with a well-developed motor tract, extends, I believe, as far as the origin of the third cerebral nerve inclusive; and it has been shown, in Case XXI., that the symptoms of spinal paralysis may indicate disease of the whole of this physiological spinal cord.

Etiology.—The only predisposing causes, or predispositions, which are very evident on reading the cases related in the first part of this essay, are age and sex. All the patients were adults, nearly all of them of middle age. The greatest age at time of seizure was sixty-two years; the least eighteen years. I would add that the oldest child developing infantile spinal paralysis recorded in my table of cases,[1] was seven years; thus leaving a gap of eleven years of life (adolescence), in which an immunity to spinal paralysis seems to exist. As regards sex, by far the greater number of the patients (seventeen) were males. This peculiarity may be logically related to the only exciting cause which is apparent in the twenty-two cases, viz., exposure to cold, or "catching cold." In four cases (III., XIV, XVI., XIX.), the action of cold upon the surface of the body is given as the cause of the attack by the reporters. In three other cases (IV., XV., XVII.), the efficacy

[1] *The Medical Record*, New York, January 15, 1874. Case by Charcot and Joffroy.

of refrigeration may be inferred: in the first case, the patient having worked hard out-of-doors; in the second, having been out on January 1st; in the third, having been repeatedly wet through. In Case I., an effort accompanied by strain of the muscles of the back is reported as a cause; in Cases VIII. and IX., the palsy and atrophy appeared as the boys were recovering from measles; in Case XII., dysentery seems to have preceded the palsy.

The inference that spinal paralysis in the adult is caused by refrigeration (or other irritation) of peripheral nerves, seems fair to draw. This view is supported by what little we know of the etiology of the disease as it occurs in young children (infantile spinal palsy); nearly every observer having expressed the opinion that peripheral irritation of nerves of relational or organic life (cold, worms, dentition, indigestion) determines the outbreak of spinal cord diseases.

To bring forward arguments to prove that peripheral irritation may set up organic disease in the nervous centres, that a paralysis *a frigore* (as adult spinal paralysis seems to be) may be due to a myelitis, would be to transgress the limits of this paper. I will merely refer to what two recent and authoritative writers say upon the subject.[1]

Prognosis.—None of the subjects died of the paralysis, with exception (?) of Case V.; the patient whose spinal cord Gombault examined dying of an intercurrent affection while she was in the stationary stage of her spinal disease. The prognosis as regards life is therefore very good, just as in the spinal paralysis of children. The small amount of danger seems to depend upon the non-involvement of the respiratory muscles, and the continued normal action of the bladder. It is well known that the majority of paralytic patients die of asphyxia, or bronchitis, or cystitis extending upward so as to set up pyelitis. The freedom from bed-sore is also a reason for the

[1] Jaccoud, *Les paraplegies et l'ataxie du mouvement*, Paris, 1864, pp. 381–386.

E. Leyden, Ueber Reflexlähmungen. Volkmann's *Klinische Vorträge*, I. Series, No. 2, 1870.

small degree of fatality attending inflammation of the motor tract of the spinal cord. The prognosis relating to the paralysis is only moderately good. Many subjects retain one hopelessly wasted limb or more. In a few cases an almost perfect cure may be hoped for (XVI., XIX., XX., XXII.). In others the first force of the disease is so great, i. e., such a destruction of ganglion cells takes place, that not any improvement occurs in the atrophied limbs. As guides in prognosis we must rely upon the rapidity with which atrophy occurs, and upon the loss of electro-muscular reaction. Even in the most promising cases a cautious prognosis, at least one made conditional upon repeated testing of the muscles with faradism and galvanism, should be given. In Case XVIII. (subject of photograph), for example, all irritability having disappeared in the muscles of the left leg, the prognosis is absolutely bad.

Treatment.—As this essay is intended to call attention to the semeiology and pathology of spinal paralysis (inflammation of the motor tract), and as it has already grown larger than it was originally intended, I think it best to dismiss this subject in a few words. The early stage, in the acute and subacute forms, should be treated by counter-irritation to the spinal region; dry cups, actual cautery, or leeches. Ergot and belladonna should, I think, be given freely, the latter so as to produce slight throat or eye symptoms. At the same time, nutrition should be furthered and constipation prevented. Pain, if severe, may be relieved by means of liniments, or simple warm wraps. In these two forms, after the active symptoms have subsided, and in the chronic form, the treatment resolves itself into attempts to restore the nutrition and force of the muscles. This is done, as in infantile spinal paralysis, by means of the galvanic current, applied in such a way as to produce muscular contractions. When improvement takes place, the muscles reacquire the property of contracting to faradism, and this agent should then be substituted for galvanism (Case XIX.). For more details upon the subject of treatment of the remaining muscular wasting, I refer the reader to my lecture[1] upon the infantile form of the disease.

[1] *The Medical Record* (New York), January 15, 1874.

My conclusions are: There is a form of paralysis in the adult very similar to the infantile spinal paralysis of children, in its symptomatology and pathological anatomy, in its etiology and prognosis. The two affections I propose to denominate, provisionally, as follows:

Spinal paralysis or Inflammation of the motor tract of the spinal cord,	In the adult.	Acute. Subacute. Chronic.
	In children.	Acute.

This disease is closely related, in the nosological scale, with acute ascending paralysis on the one hand, and to progressive muscular atrophy, and labio-glosso-laryngeal paralysis, on the other.

DISEASES OF THE MAMMARY GLANDS DURING LACTATION.

By SALVATORE CARO, M.D.

Read November 19, 1874.

The most remarkable phenomena regarding lactation following delivery are milk fever, enlargement of the mammæ, and the milk secretion. This enlargement and secretion of the breasts, although often observed at the beginning of utero gestation, cannot be considered as the true process of the milk secretion necessary to the support of the infant, but rather as a sympathetic uterine action, considered by many writers as a pathognomonic symptom of gestation.

The three above-mentioned phenomena, which are often ushered in simultaneously, are observed between the third and fourth days (seldom later) after delivery, when the pulse becomes accelerated, the temperature rises, the patient feels thirsty, suffers intense headache, with occasional chill, the lochial discharges disappear (to return at the end of this stage), the mammæ become enlarged, very hot, painful, and sensitive to the touch, indicating the secretion of milk as having commenced to take place. This febrile stage, which is considered by all writers as not only normal, but necessary to the secretion of milk, has been called milk fever, and lasts from twenty-four to thirty-six hours, leaving, at its disappearance, the properly-formed milk secretion; and with it the duty and high office allotted to woman of nursing her offspring.

In reference to this high office, however, before entering into its discussion in this paper, I beg leave to say that, in

spite of all the dictates of Nature and the teachings of those philanthropists predisposed to assert speculative notions, it is often observed that the delivered woman not only cannot perform the duty to which the majority of mothers are naturally compelled, but that their persistence in such obligations may be fruitful of harm to both mother and child, as it is often observed that a weakly-constituted mother, with a chronic malady, or a marked predisposition to such, or tuberculosis, is the counter-indication to lactation. In fact, delicate women, from the beginning of lactation, feel a sense of general extenuation of suffering at the thorax, shoulders, and hypogastrium, loss of appetite, rest, and cheerfulness; become fretful and peevish, and, if the milk does not cease to secrete, a general emaciation will soon follow, and compel them to abandon the task.

Another class of women, losing their strength in consequence of excessive labor, mental excitement, extra care given to their children, particularly at night, having no opportunity of seeing them through the day, or by reason of scanty nutrition, are compelled to relinquish their natural obligations. Fashion, also the ambition of preserving a fresh appearance, and many other reasons, often prevent the mother from nursing her infant, and compel the physician to guard her against such a procedure; but when the woman is healthy, and surrounded by all the appropriate hygienic conditions, nursing is a paramount duty which the mother should perform without fear of any ill effects upon the constitution, as it rather imparts vigor and strength; it being often observed how, under such circumstances, a woman will look fresher, enjoy better health, and lose all tendency to various affections.

As I have just said, the mammary glands are predisposed to a fluid secretion, varying in quantity and quality from the beginning of utero gestation to confinement, undergoing various changes before reaching a perfect milk composition; the true secretion, however, does not take place until a few days after confinement. From the first to the third day the glands remain almost in the same condition as before confinement,

secreting but little semi-aqueous fluid, of a whitish color; from this period, colostrum and a milky fluid, of yellowish, lemon color, thicker than the former, are freely secreted until the fifth or seventh day. At this stage, the phenomena indicating the milk secretion having passed away, the secreting fluid gradually becomes clearer until the tenth or fifteenth day, at which time it has assumed the proper color, consistence, and qualities, of pure milk. During puerperium, or at any time while lactation continues, women may be affected in the mammæ in various ways—namely, with anomalies of secretion, galactocele, sore nipple, engorgement of the glands, phlegmonous inflammation, mastitis, and painful nursing. In reviewing these various affections, I will commence with the least dangerous and explain.

These anomalies may be divided into agalactia and galactorrhœa.

Agalactia, or the absence of milk, which is supposed to be independent of the will of woman, may be complete or incomplete. It is complete when the milk totally ceases to secrete, and incomplete when partially, or not enough to support the child. Moreover, agalactia may be primitive, as when caused by an incomplete development of the glands, atrophy of the adipose tissue, or by defective nervous vital energy of the same; and consecutive or accidental, which may be caused by defective or unhealthy nutrition, want of constitutional power, acute or inflammatory diseases, organic maladies, exposure to cold, astringent applications to the breasts, fever, eruptive diseases, excessive discharges, whether lochial or leucorrhœal; passive hæmorrhages, either during pregnancy or after labor; moral influences, as sudden fright or terror, anxiety of mind, unpleasant news suddenly communicated, grief, depressing passions and emotions, disappointment, vexation, anger, and many other unexpected causes. Either of these forms of agalactia may be recognized by the flaccid condition of the mammæ; or by the emaciated appearance and continuous crying of the child. Of the above anomalies none are invariably a detriment to the mother, except the acci-

dental, particularly when the arrested secretion affects the crasis of the blood; but they are always very injurious to the child, as they deprive it of the proper nourishment.

The treatment of primitive agalactia is limited, and affords but little benefit; nevertheless, that by galvanism, proposed and experimented on by Aubert and Becquerel, and recently the faradaic current, indicated by many others, have rendered valuable service to medicine and the sufferers. In accidental agalactia, the topical remedies recommended by Desormeaux and others are the fermentations of decoctions, prepared with anise and fennel root, cascarilla bark, and also poultices of lentils. The most reliable remedy, so well known and so much used by and among Italian women, is the poultice from the leaves of the *Ricinus communis* (castor-oil plant). In the absence of the leaves, poultices of the powdered beans will replace the former. The following case, as an illustration, will justify its narration:

Miss B., aged twenty-four, healthy and robust, with well-developed breasts and nipples, primipara, in the winter of 1860, while in the seventh month of utero gestation, became affected with small pox of a very confluent form, through which she lost considerable strength, and the breasts, which were full of lacteous fluid from the beginning of pregnancy, during her sickness withered away, leaving no signs of milk. At full term she gave birth to a healthy boy, but when put to the breast no milk was found. For several days the trial was occasionally repeated, in order to stimulate the gland and start the secretion, but to no effect. Nourishing food with plenty of beef tea was given, and, though in consequence she fully regained her strength, no milk appeared. On the fifteenth day after confinement, all other efforts failing, a trial was made by the application of castor beans. It being winter, and impossible to obtain leaves either fresh or dry, but remembering the good effects secured by the poultice of castor beans in a similar case during the winter of 1848, I determined to make the trial, and, after the third day's use, I was agreeably surprised by the appearance of milk, which continued to the end of lactation.

When the form of agalactia has affected the blood and otherwise undermined the system, its treatment should consist in removing the cause, and restoring strength by tonics, good nourishing food, and country air.

Since writing the above, I have ascertained that a preparation called ext. flu. Palma-Christi has lately been prescribed internally by several physicians with good result; but being of an insipid and disagreeable taste, and occasionally disturbing the digestive functions, I would prefer the local rather than the internal use.

During the above period the mother may become the subject of the second anomaly, galactorrhœa, or the too free secretion of milk.

Any young and healthy woman, under the most favorable circumstances, may become the subject of what is termed primary galactorrhœa, particularly when the infant is too weak to nurse. This form is considered physiological; still carelessness may produce pathological changes of a rare character, proving injurious to both mother and child.

To prevent primary galactorrhœa from producing any ill effects, three things are necessary to be observed: 1st. The physician should ascertain whether the woman has a well-developed nipple, or whether it has been flattened through tight lacing; 2d. The infant must be healthy, without deformities in the buccal organs, such as hare-lip, imperforated nostril, or tongue-tied; and, 3d. He should ascertain whether the mother intends nursing or not.

If the nipple is naturally short, a daily traction should be made upon it, which may be easily accomplished by pulling it forward with the fingers, and in a short time it will be made sufficiently long for the purpose. If a consequence of pressure, all that is required is to removet he cause; and, if it is intended to nurse, to put the child to the breast four or five hours after confinement. The advantages of early nursing are twofold: 1st. The few particles of formed milk being mixed with the colostrum, it becomes necessary that it be suckled away in order to relieve the infant's bowels from the me-

conium; 2d. The breast being soft and of normal heat, the infant finds no objection or difficulty in seizing the nipple with the gums, and making it its first play toy and the vehicle of its food; while, by waiting until the secretion is established, the enlargement and tension of the gland embedding the nipple within the projecting areola, and leveling it with the gland, grasping it would be very difficult for the child, and suction totally impracticable. Moreover, the milk accumulating within the glands, unless discharged by a spontaneous dribbling, mechanically removed or therapeutically arrested, would cause engorgement and abscess of the breasts. If it so happen that, independent of a strict observance of all rules, the milk continues to accumulate, the glands must be relieved, either by mechanical contrivances or external remedies. If artificial nursing becomes necessary, it may be accomplished either by a stronger or older child, a woman, a pup, or by pumps of various kinds. In the choice of an instrument care should be taken to select one approaching nature. My preference through experience is for the instrument of Mr. Haggerty, which is the best adapted to afford the same relief as that imparted by the mouth of the child. If artificial means fail, it becomes imperative to prevent secretion, and the duty of the physician is to institute a therapeutical *régime* by diuretics, laxatives, iodide of potassium, and such local remedies as camphor, mint, stramonium, belladonna, etc. The remedy adopted by myself is composed of: ℞. Ext. bellad., ʒ iij, gum camph., ʒ ij, ungt. stramoni, ℥ jss, M.; to be spread over the affected breast every four hours, keeping it covered with oiled silk. By so doing I have either modified or totally arrested the secretion in one or both breasts almost at will, thus preventing engorgement and abscesses, as may be seen by the following case:

In 1866, being engaged to attend Mrs. R., aged nineteen, upon examining her breasts before confinement, the left only was shown me, the right concealed through bashfulness. After delivery nursing went on without apparent difficulty until the seventh day, when she was taken with chills, fever, severe

headache, and distressing pains in the right breast. Looking for the cause, I found the periphery of the right thorax, far above the clavicle, greatly swollen, the breast without nipple, and only with a small portion of the areola, both having been destroyed in childhood by a severe burn. The use of the above prescription caused a gradual decrease of the secretion until the tenth day, when it entirely ceased without extending its influence to the right breast, salivary glands, or pupils.

We have besides two other forms of galactorrhœa, normal and abnormal; normal regarding the nutritive quality of the milk, but abnormal in its quantity. As the free secretion may debilitate the mother, it is necessary to correct it, which may be easily done by preventing the stimulation of the glands by too frequent nursing, and by the internal use of the infusion of rhamnus alaternus. The second form, which is called by some writers mammary diabetes, is abnormal in quantity and quality; in quantity for its excessive secretion, and in quality for its deficiency of nutritive particles and abundance of water. As this form affords no nourishment to the child and debilitates the mother, the moment it is discovered means should be taken to remedy it, either by tonics, country air, good nourishing food, and numerous other things. But as this affection is generally associated with the product of some morbid condition of the system, properly speaking, not belonging to the puerperal stage, I will refrain from enlarging upon it.

Galactocele, or the enlargement of one or more of the milk ducts, which is caused by the sudden accumulation of milk, is one of those affections which occasionally follow parturition, and, as it may be mistaken for mastitis, I feel it my duty to say a few words concerning it. Of this affection, as it is described by Scanzoni, we have two forms, incomplete and complete; incomplete when the lactiferous ducts are partially obliterated, and complete when totally so. In the incomplete galactocele, the ducts being partially obliterated, the milk slowly secreting will cause no other discomfort save a temporary dilatation of the canals, enlargement of the same, and a slight pain. In the complete galactocele, the ducts being

totally obliterated, and the secretion wholly obstructed, as Scanzoni observes, its internal pressure will cause the rupture of one or more of the canals, the effusion of the milk into the cellular tissue, and an accumulation of this fluid into smaller or larger cavities, where it will gradually thicken and determine a suppurative inflammation of the neighboring tissues.

The enormous cystic enlargement of the breasts with circumscribed fluctuation, without local heat or pain, redness of the skin, chills, fever, or any inflammation, are the differential symptoms of diagnosis between galactocele and mastitis.

The treatment depends upon emollient poultices, camphor, belladonna, and an early opening of the sac. The above remedies being too slow in their action to prevent the rupture of the ducts, consecutive effusion and suppurative inflammation, I have been in the habit of opening them as early as practicable, and allowing the contents to run out freely; but, with this procedure, I must confess to have been more than once disappointed, in consequence of the difficulty experienced in healing the milk fistula, which has continued to run to the end of lactation, to the great detriment of the mother and child, by the continuous loss of milk. However, observing the necessity of an early opening of the ducto-cystic tumor, instead of incision, I have instituted aspiration, which is less painful and more efficacious. Under this method the risk of the rupture of the walls of the ducts by distention, or the milk fistula, is prevented, and the cause of the obstruction is removed; nursing is not interrupted, and the mother is spared from long suffering. If by a single aspiration the sac is not emptied, a second, third, or even more, are not objectionable, as this process is almost painless, and exempt from risk.

The instrument and *modus operandi* are simply an India-rubber syringe armed with the trocar needle, its plunging at the most prominent point of the cyst where the fluctuation is distinctly felt, and the aspiration of its contents in the usual

way. In a case where I operated last June, the success was flattering, a single syringeful being sufficient to evacuate the sac. On withdrawing the needle, a small particle of colostrum forming a cast, with shreds of coagulated milk, was found inserted in its point, evidently the cause of the obstructed canal, which once removed, the milk resumed its natural flow, and the small opening made by the needle healed in one day by first intention.

Excoriation, fissure, and ulceration of the nipple, are affections to which primiparas are more subject than multiparas. However slight those affections may be, they yet are distressing and painful, and, if neglected, may be followed by very serious consequences. These affections, as a general rule, commence from the first, second, or third week after confinement, rarely beyond that period, and are caused by the frequent suckling and pinching at the nipple with the gums of the child, the infant's saliva, exposure to cold air, which acts like an irritant upon the moistened surface, negligence of cleanliness, and primiparas with delicate skin and short nipple. Their premonitory symptoms are a superficial abrasion of the nipple, with jerking pains while nursing, quite tolerable at the beginning, but ending with a momentary burning sensation. As the excoriation advances, the nipple, becoming slightly swollen, assumes a reddish hue, and, when the child is put to it, the pain is so intense that the mother is thrown into paroxysms of chills, accompanied by nausea and vomiting. The suffering from superficial fissure is the same as excoriation, but as it ulcerates and extends deeper down, affecting the areolar tissue, the pain becomes so intense and constant that the mother will refuse nursing her child, allow the glands to become engorged, and suffer the consequences of an abscess, very likely followed by the total loss or retraction of the nipple. The prophylactic treatment depends mainly upon the rubbing and washing of the nipple for a few weeks before confinement, with a stimulative or astringent lotion, as tincture of myrrh, or cold rose-leaf tea.

The *post partum* prophylaxis on those with short nipples

and a tendency to soreness, should be, pulling them with the fingers gently and steadily one or two moments before nursing, and after nursing to wrap them in strips of linen wrung out of flaxseed tea. If the nipple becomes sore the child should be prevented from nursing without a shield (and I find Haggerty's nipple shield the best adapted), besides applying, locally, a detergent and mucilaginous lotion composed of equal parts of quince-seed mucilage and compound tincture of benzoine. Should the nipple be deeply ulcerated or fissured after the above application, strips of membranous plaster, perforated in various points to allow the milk to pass, should be laid over. By this method I have succeeded in relieving and hastening the cure in many cases of the most aggravated form, as may be seen by the following:

Mrs. J. M. S., aged twenty-eight, multipara, with breasts and nipples uncommonly large. Her child, eight months old, with four teeth, in a fit of anger while nursing bit the nipple and almost severed it. Obtaining no relief from her own medications, and unable to suffer longer, she called me in attendance. On my first visit I found the nipple deeply ulcerated, the breast largely engorged, very painful, and with the general symptoms of an inflammatory process. To relieve her I applied the above mucilaginous lotion, strips of membranous plaster, and ordered the nursing to be done through Haggerty's nipple shield. In a few days the breasts became empty and the nipple healed.

Negligence to the above affection will be soon followed by engorgement, phlegmon, and abscess of the mammæ, which are divided into mechanical, and passive, active, and inflammatory.

The mechanical and passive engorgement affecting but one mamma does not progress so rapidly, in consequence of the constant dribbling of the affected breast while the healthy one is nursing; but if the nipples of both are deeply fissured, the total inability to nurse, distending both breasts, will cause active engorgement, and assume the aspect of phlegmonous inflammation, which, once established, acts like cause and effect, producing the most distressing affection during lactation.

In addition to the cause producing fissure and ulceration, engorgement may be brought on by the following: irregularity of nursing, nursing more from one breast than the other, sudden discontinuance of the same, occlusion of the lactiferous ducts and nipple by coagulated colostrum or milk distention, rupture of the ducts, and by the flux of blood occasioned by pressure upon the adjacent cellular tissue, by hyperæmia and exudation, exposure, moral affections, dragging down of the breasts, dietetic imprudence, and very often by a natural predisposition.

The moment any of the symptoms of these affections are ascertained, means must be taken to prevent their continuation, either mechanically, as by Haggerty's instrument or nipple shield, as above shown, or therapeutically, externally, or internally. The external remedies most used are equal parts of olive oil and ether, camphorated oil with oil of mint, belladonna ointment, emollient poultices of flaxseed meal, althea and lettuce leaves. A remedy from which I have obtained valuable relief for many years, as I will directly show, is the equal parts of fluid extract of belladonna and crocus sativus, gently rubbed upon the breast three times a day. Care should be taken, though, to wash the nipple before nursing. Mrs. H., multipara, with good breasts and nipple, being carelessly habituated to nurse from the left more than the right breast, the latter becoming painful and swollen by the accumulated milk, threatened inflammation, from which she was soon relieved by regularity of nursing, and the external application of the above remedy. The internal remedies are limited to salines, laxatives, diuretics, diaphoretics, and iodide of potassium. The infusion of rhamnus alaternus, which is highly recommended by Dr. Orioli, much used among Italian women, and of which I have had long experience, arrested the inflammatory engorgement of the breasts of Mrs. A., aged twenty-one, primipara, who had neglected nursing for two days, in consequence of extreme pain produced by fissures.

Returning to the last of the causes producing engorge-

ment, namely, a natural predisposition to mammary abscess, I may say that this notion is refuted by all and tolerated only by few. In their opinion its regular recurrence is based upon the susceptibility of the mammary glands to swelling and inflammation during lactation by the least irritative cause, and by a lymphatic temperament with fretful and nervous predisposition.

During my practice, having observed these abscesses in perfectly healthy women, with calm and easy dispositions, surrounded by all the comforts of life, I am inclined to differ with them, and establish as a cause the occlusion of a milk duct by coagulated colostrum or milk, which, after the first occurrence, unless removed, at a subsequent nursing may produce the same effect as before. The two following cases, one *a priori*, and the other *a posteriori*, may corroborate my statement.

In the *a priori* case, the subject was a Mrs. S. T., aged twenty-three, primipara, who on May 27th gave birth to a female child. A few hours after confinement she put her child to the breast, and continued nursing her as often and as regularly as she could naturally do. The milk went on flowing freely until the eighth day, when she called my attention to a kind of unpleasant sensation in the right breast. On examining it, I found a small ridge about two inches long, of the size of a large pencil, situated outwardly and in its middle part with a slight sense of pain when the child was nursing. After thorough inspection, I ascertained that I was dealing with an incomplete galactocele, as before described. I prepared myself for the emergency, and ordered camphorated oil to be rubbed on the affected breast, in order to scatter the accumulated milk and diminish secretion. My efforts were to no purpose, as the enlargement increased, until it was the size of a large lemon. Although there were no inflammatory symptoms nor distressing pains, having defined a distinct fluctuation, fearing rupture of the canal and its consequences if left alone for a while longer, I determined to evacuate it, and accordingly on the third day I aspirated it with a common India-

rubber syringe. After drawing four ounces of milk, the sac having totally collapsed, and no more fluid coming, I withdrew the needle, and to my delight, as I had predicted, I found inserted in its point a small colostrum cast with shreds of coagulated milk; the opening was closed with adhesive plaster, healed by first intention, and nursing continued without interruption.

The *a posteriori* case was that of Mrs. M., who, at each of her previous confinements, had abscess of the right breast. Attributing the occurrence to faulty care on the part of the accoucheur, in her coming third confinement she determined to make a change, and accordingly, when about the seventh month of utero gestation, engaged, and apprised me of the facts. To ascertain the cause, I examined the nipple and breast, and found both well developed and without deformity, except a small tumor, about the size of a common pea, situated in the internal contour of the right breast, about two inches from the nipple, from which she recollected her first trouble commenced. As she was anxious to have something done, I instituted a prophylactic treatment by keeping the breasts loose and painting the tumor three times daily with a weak solution of tincture of iodine and fluid extract of belladonna. In due time she gave birth to a healthy boy, who was put to the breast a few hours after confinement. Mother and child did uninterruptedly well, until the ninth day, when she felt a dragging sensation at the breast, with its gradual filling up, which continued until the fifteenth day, when it was lanced, and about two teacupfuls of fluid, more of milk than matter, came out. The progress of this engorgement was so astonishingly rapid that the antilacteal remedies had no effect whatever, leaving no alternative but the poultice to help suppuration, and lancing in due time. Nursing from this breast was necessarily abandoned, healing progressed slowly, and the old tumor remained when the opening was closed.

Of course I lost her patronage and that of her family, but afterward learned that on her fourth confinement she had the dreaded abscess again in the same breast, and meeting her

husband a few days since, he informed me of the same occurrence having taken place in subsequent confinements. I am therefore forced to think that the removal of the cast by aspiration in the former case, the succeeding free flow of milk, the sudden relief, the instant cure, and the interrupted nursing—all bring me to the conclusion that the cast obstructing the lactiferous duct was the active cause of the galactocele, and its accidental removal the permanent cure; while, in the second case, the unexpected abscess at the first lactation, the existence of the small tumor after healing, the regular reoccurrence of abscesses from the same breast and locality, and always with the same symptoms, suggest to me that a cast having occluded the lactiferous ducts produced galactocele, engorgement, and abscess; otherwise, the inflammation of the first abscess affecting the internal tunic of the lactiferous ducts caused a permanent occlusion of the same, and naturally a regularly recurring galactocele, and its consequence at every subsequent confinement.

Of all the above-mentioned mammary affections, mastitis is the most distressing and most difficult to relieve. This affection originates either from the subcutaneous cellular tissue, from the whole gland, or from the cellular tissue between the pectoral muscle and the body of the gland. The first two of these affections are observed only at the epoch of the functional activity of the glands, and are produced by all the causes mentioned in the glandular engorgement. The third, originating from a membrane under the gland, may occur at any time during lactation, and may be caused by dietetic imprudence, cold, scrofula, arthritis, and traumatically.

The symptoms accompanying the former are ushered in by chills, fever, and lancinating pain, in one part or all over the breast, and are felt more keenly when the child is put to it, or when under pressure. At the seat of this pain is found a limited engorgement which gradually increases over the whole mass, and shortly after a sort of doughy feeling is observed on palpation, indicating the subjacent pus ready to be let out by incision. The pain of the third form is felt from

the beginning quite deeply, and, in addition to the above symptoms, is accompanied by swelling of the axillary glands, great thirst, scanty urine, constipated bowels, total loss of appetite, insomnia, and occasional cerebral phenomena. Moreover, a hard knot or nucleus, from which this affection originates, is found deeply under the gland, easily recognizable in the beginning when the breast is not engorged by its nodulated surface. As this affection progresses, the nucleus or nuclei increase in number, the pain is felt sharper and more general, and the breast which was not hard in the beginning becomes very much so as the disease advances, the whole train of symptoms become very much aggravated by the total cessation of secretion, and the formation of pus either in one grand sac, or in various points constituting one or more abscesses.

As regards prognosis, mastitis is never attended by death except in very rare cases, when the system has been very much undermined, or by pyæmia; but it is always attended with severe pain, and if the abscess has affected several milk-ducts, each one forming a separate fistulous sinus, healing is more obstinate and suffering more protracted.

The treatment of the first two forms of these affections is the same as in engorgement. In the last form, as nursing, rubbing, and other manual interferences, are attended by distress, and often aggravate the case, care should be taken in their use. Laxatives, diaphoretics, and acids, are the best internal remedies. If the local affection is accompanied by general inflammatory symptoms, bleeding or leeching is at once to be resorted to. Evaporation with aqueous extract of opium, fluid extracts of belladonna and crocus sativus, poultices of hyoscyamus, lettuce, althea leaves, and flaxseed meal, are the best topical applications to facilitate suppuration and allay pain. The moment fluctuation is ascertained, an early opening is highly recommended; owing to the depth of the affection, though, fluctuation is seldom felt before the lapse of fifteen days or more, and even then it cannot be detected with decided precision; but it can be suspected from the enormous size and smooth-

ness of the breasts, the bluish hue of the skin, and the œdema of the surrounding tissues. If with such evidence the real presence of pus is not detected, plunging the knife deeply into it will soon reveal its existence and justify the procedure. If there are more than one abscess, each should be opened, allowing the matter to discharge by keeping the sac collapsed, by pressure, with strips of adhesive plaster. With the opening of the abscess, if the collection of pus is in one large sac, the difficulty generally is ended by its evacuation; but if in more than one, and the destruction made upon the neighboring tissues and lactiferous ducts, or the hypertrophy and induration of the gland too great, healing will be attended with more difficulty in consequence of an endless number of fistulous sinuses which by their burrowing tendency are very obstinate to heal, and when cure has taken place the breast is left quite hypertrophied with retracted nipple, and the constitution of the patient greatly undermined. To help the absorption of the indurated tissue, it is necessary first to invigorate the mother by tonics and animal diet, and the use of gradual compression by corseting, and prepared sponges. This affection seldom occurring, its consequences are not fully appreciated, and in order to give you a slight sketch I take the liberty of intruding upon your already overtasked patience a report of a case which was rather singular in the manner of its occurrence:

Mrs. M. M., aged thirty, primipara, of lymphatic temperament, on May 5, 1866, fifteen days after confinement, while enjoying the courtesies of friends, had a fit of laughter, and at once felt a slight chilly sensation, followed by a severe pain in the right breast, which increased by nursing. The following morning, on being sent for, I found her suffering with shooting pains deep in the breast, dry and hot skin, with pulse 120, and tongue furred and dry. Examining the breast, I found a hard nodule the size of a bird's egg deeply embedded in the centre of the gland and very sensitive; being the first case of the kind I had ever seen (and fortunately the last), I doubted its nature; as there was no defect in the nipple, nor accumulation

of milk in the glands, my attention was directed to the local pain and general symptoms. Accordingly, I prescribed saline draughts and leeches; none of these producing any relief, anodyne poultices were applied to allay pain and facilitate suppuration. After eighteen days of constant excruciating pain, finding a spot indicating fluctuation, I opened it, and a considerable amount of greenish pus was discharged. With the opening of it I thought the difficulty was controlled, but was disappointed by the appearance a few days after of numerous little openings, riddling the breast like a sponge, which discharged milk and matter for forty days, resisting all sorts of medications, yielding only to constant graduated pressure by corsets, leaving the nipple retracted and the breast quite disfigured.

The last two of the before-named affections are pain in the breast and painful nursing. The first, being caused by keeping the child too long from nursing, is temporary and not alarming, save for a certain degree of discomfort, fullness, and slight pain at times when the milk is forced out in jets, or drops by involuntary or spasmodical contraction; and if during this temporary spasm the child is put to the breast the sense of discomfort is aggravated and changes to pain, particularly in the opposite breast. To prevent its recurrence, nothing but careful attention to regular nursing is necessary.

Painful nursing is an affection to which some women are subject, especially those of weak and nervous constitutions; and, as it often originates in nervous debility, a tonic treatment, associated with anodynes, generous diet, and country air, will be sufficient to insure relief. Obstinate cases resisting the above treatment will yield only to the weaning of the child.

If by the above remarks I have not succeeded in conveying an exact idea of the title of this paper, I beg to be excused, for, *Quid potui facere fœci faciant meliora potentes.*

A CONSIDERATION OF CERTAIN SYMPTOMS ASSOCIATED WITH MORBID CHANGES IN THE MEDULLA OBLONGATA.

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Read January 7, 1875.

I PROPOSE, this evening, to discuss the symptomatology of certain obscure neuroses centred in the medulla oblongata. It will be almost a necessity to consider the relationship between symptoms and central morbid changes, in a purely theoretical way, for post-mortem examinations of such cases are rare. Trousseau, Duménil, Wilks, and one or two others, have made autopsies, but, with these exceptions, nothing has been done to increase our knowledge of the pathology of these interesting diseases. I am sure many will agree with me that the medulla is by far the most important part of the cerebro-spinal system, forming, as it does, a sort of figurative keystone in the cerebro-spinal arch, changing the direction and character of nervous impulses that pass from the brain downward. Its chief importance lies in the fact that several of the principal cranial nerves, viz., those presiding over respiration, deglutition, and speech, as well as several others, take their origin or pass through some part of it to supply various important organs. When we consider the duties of such a nerve as the pneumogastric, we may at least realize the variety of functional disturbances that may follow when there is a point of disease at its root in the medulla. Although certain well-regulated and demonstrable diseases are centred in this organ, there are many that are not so easily described. Certain general neuroses involve this tract as well as other parts, but we

recognize the invasion of the medulla by certain disturbances of physiological functions that are characteristic.

If we examine a section of the medulla beneath by the microscope, with an objective of comparatively low power, we will see how closely in contact lie the roots of two very large nerve-trunks, the hypoglossal and pneumogastric; we will also see the crossing of certain fibres from one half to the other of the medulla, so that there is a united action of the two halves of the organ. We would see the two important bodies known as the olivaries, and find certain reënforcements of fibres going from these little tubercles to the hypoglossal roots. With a higher power we would detect a collection of caudate cells at the nuclei of these nerves, and witness their intimacy and intercommunication.

Lockhart Clarke, whose investigations certainly rank as high as any, and to whom the student owes much for his knowledge of nervous structures, has shown the close relation of the nuclei of the hypoglossal, pneumogastric, spinal accessory, facial, and fifth nerves. He has demonstrated the existence of a set of cells, common to these nerves, and shown that, when there are disturbances of one or more of the functions of organs supplied by nerves from the medulla, destruction of some of these cells will exist, and that, with destruction of all of the cells, there are general symptoms of loss of deglutition, expression, speech, labial muscular power, etc.

An interesting point in the study of these forms of disease is, the anatomical relation of the thoracic and abdominal organs with the medulla. Starting with the pneumogastric and its branches, we trace its distribution to the larynx, lungs, heart, stomach, and large intestine. We are enabled to demonstrate its communication with the sympathetic; we dissect, and find the ganglia it forms with fibres from this last nerve.

Dittmar[1] located the vaso-motor centre in the medulla, and proved that it consists of a well-defined spot on the inner side of the facial nerve, at its exit, and is composed of gray matter, and is three millimetres in length, one and a half in

[1] Bericht der Kais.-Sächs. Gesellschaft der Wissensch., 26 Juli 1873.

breadth, and is about two millimetres from the raphe. This tract was found to be the reflex centre for the vessels. The contiguity of the other nerve-roots to that of the facial and consequently to this centre will, from disease or functional impairment of their conductivity, lead to transmitted irritation to this locality.

It has been found by some German investigator, whose name now escapes me, that, when a frog is struck smartly upon the abdomen over the solar plexus, circulation will be stopped, and the cavities of the heart will become so gorged with blood, from paresis of the organ, that it will cease to pulsate. I think the inference is clearly proved that the pneumogastric fibres are affected as well as the sympathetic, and that the irritation sent to the medulla is reflex. This physiological fact is given particular interest when we consider that irritation of the vagus at its periphery arrests the heart's action very materially.

The disturbance of circulation, particularly in the lungs, during the course of some convulsive seizures, is another pathological state which shows the interesting physiological relation of these nerves.

The distribution of the hypoglossal, facial, and the glosso-pharyngeal, naturally lead us to look for neuroses of the tongue, facial muscles, and those of deglutition. The spinal accessorius, a nerve generally involved, when irritated at its root, produces, as one of the results of such stimulus, a contraction of the cervical muscles, and, in turn, there are constriction of the blood-vessels of the neck, and intra-cranial changes in circulation.

The consideration of pathological change in the medulla is interesting in the extreme, because of the exhibition of striking evidence of alteration of physiological functions. Abnormal variations in circulation, atrophic, or hypertrophic changes of the nervous substance of this part, are expressed by a series of characteristic symptoms.

Changes in the nuclei of the pneumogastric are followed, as we would suppose, by pulmonary, gastric, and intestinal

symptoms. The asphyxia, which, in an exaggerated state, is followed by coma, is the most pronounced type of such a neurosis.

Van der Kolk[1] found pneumonia in a patient with disease of the medulla oblongata. In this case the heart was greatly dilated, and the mucous membrane of the lungs very red, probably owing to obstructed circulation. The stomach was large and distended, as was also the transverse colon. The pathological points of interest were yellow degeneration of the nuclei of the vagus and accessorius. In many cases, probably all, if examinations could be made, there would be found the same pulmonary engorgement.

In many instances there is retention of mucus, and paresis of the muscular fibres. Hæmoptysis is a symptom which is also undoubtedly due to paresis of the vaso-motors, and passive congestion. Cough is a frequent symptom of these neuroses.

Various defects in articulation appear where the hypoglossal roots are the seat of disease. Where defects of speech are dependent entirely upon loss of function in the tongue, the patient will find it almost impossible to pronounce the letters *u*, *c*, *t*, *d*, *j*, *n*, and several others. Should there also be trouble at the roots of the facial, other letters may be included, as the lips are implicated as well as the tongue. This is the case in the glosso-labio-pharyngeal paralysis of Duchenne. This disturbance of speech should not be confounded with that of aphasia, where the lesion is situated in the cerebrum.

Changes in the nuclei of the seventh nerve will be followed by paralysis of the muscles of expression, or by spasms. The auditory is occasionally affected, when hallucination of hearing will be present.

Undoubtedly the abnormal secretion of saliva, so constant in glosso-pharyngeal paralysis, is due to a lesion of this nerve. Electrization has been shown by Schultze to effect a reduction in the amount. A peculiarity of the loss of conductivity of

[1] Works, New Sydenham Society, Edition 1859.

this nerve lies in the fact that no change in expression follows emotional excitation. Paralysis of the muscles of the pharynx, and defective powers of deglutition, also follow disease of the hypoglossus.

As I have said, the limited number of autopsies has prevented us from making any thing like a classification of diseases that have their centre in this part of the nervous axis. The functional varieties are rare, so much so that there is only one condition that has been described by itself, and Lichenstien, who defined it, was unable to tell whether it depended upon organic changes or not. This condition he called *laloplegia*, and described three cases—two lived, and one died. The principal symptom seemed to be paralysis of the muscles of deglutition and speech. He believed it to be due to mental emotion. One of his cases he thought to depend upon a lesion at the upper part of the medulla, near the origin of the vagi, which are finally involved.

Dr. Edward Fox,[1] of Bristol, describes a case very much like those of Lichenstien's: "A boy five years old, bright and intelligent in mind, but having always been extremely delicate, and liable to faint away on the least exertion, is the subject of the following symptoms: He has never been able to swallow any thing solid since his birth. He swallows liquids well. The palate is equal, and is moved on inspiration. He protrudes the tongue well and straight. He cannot pronounce *d* or *k*. There is a little diminution of the motor power of the right facial muscles. No others are paralyzed. Septum nasi is pushed over toward the left side, and nearly fills up the left nostril. Cries, laughs, and coughs, much as other children."

As epilepsy is as well a disease of other parts of the brain, I will not allude to it. Certainly the interesting points connected with the reflex excitations producing the attack, the subsequent respiratory derangement, and the contractions of the muscles of the neck, may be studied by the neuro-physiologist.

[1] "*The Pathological Anatomy of the Nervous Centres.*" By E. Long Fox, London, 1874, p. 234.

It has been my good fortune to meet with two cases of a disease purely functional, having its seat in the medulla, and characterized by an extremely interesting train of symptoms:

I.—Mrs. H., aged thirty-five, married. Three years ago she was first disturbed by uncomfortable feelings of faintness, troubled respiration following moderate exercise, and occasional efforts at vomiting. She had, at this time, what she called "fits," and they were very much like those she has now. She never had rheumatism, or any other disease likely to be associated with or followed by cardiac affections. She has never had uterine trouble, and had one child, the labor being natural. She has led a healthy, out-door life, going about with her husband, who is a circus-performer. Her habits are good, and there is no history that will lead to the discovery of a cause. She has had the "fits" every four weeks, when, for several days, there would be all the troubles of respiration I have alluded to.

Three weeks ago, while in a store, she was suddenly deprived of muscular power, and fell upon the floor, powerless to do any thing. The muscles of the upper part of the body, she says, were violently convulsed. After several minutes she spat up a quantity of frothy blood.

I saw her first two weeks ago, when she came to me; she was then breathing very rapidly, and her heart throbbed violently. Her face was flushed, and she was very much agitated, as she said she feared an attack. I quieted her, and obtained the above history. Dr. F. H. Rankin and myself both examined her chest, but could not discover any abnormality, either by percussion or auscultation, and we judged the trouble to be functional. But, strange to say, the patient was not at all anæmic, and apparently in perfect health. The pulse was irregular. Further examination failed to reveal any organic trouble in other parts of the body. She complained that the attacks were aggravated by excitement and worry, and were often brought on by emotional disturbance. She had vomiting attacks, which came on even when digestion was normal. There was no nausea; cold water taken into

the stomach would produce vomiting as well as the convulsions. The latter, she said, were always preceded by a "tightness of the chest," as she expressed it, and a feeling of pain all over the thorax. She also stated that, though without control of the muscular spasms, she was cognizant of every thing about her.

About a week after, I was called to see her; she was seated in a low chair, her face was very much congested, and her speech was thick and "leathery," the linguals being badly pronounced; there was nystagmus, and she expressed a fear of a coming attack. There was great uneasiness, and she moved restlessly about. She complained of a sense of weight upon her chest. Her arms were extended convulsively. The muscles of her neck were tense, and the head was drawn downward, and agitated by semi-tonic spasms; there was momentary unconsciousness. Her inspirations were deep and quick; for several seconds her heart seemed to have stopped beating, and respiration became interfered with. There was exquisite hyperæsthesia over the whole upper right side; a touch of the finger on the skin produced pain. During the seizure the eyes were rolled upward. In about three minutes from the beginning of the attack, she opened her eyes and spat out some bloody saliva; this was coughed up; there was no biting of the tongue nor soft parts of the mouth, and the blood was undoubtedly from the lungs. She had several of these attacks, each like the first, but there was no more bleeding.

During one of the seizures, while apparently unconscious, she answered questions, and gave directions to her servant. A marked feature of the attack was the existence of trismus during the time when the convulsions occurred.

The second case I saw, in consultation with Dr. Stevenson, of Poughkeepsie, presented some of the features of this.

II.—Mrs. ——, about twenty-two years old, married several years. Before her marriage, for many years, she was subject to convulsions, which were called epilepsy by her family and former medical attendants. At intervals she

would be seized in much the same way as Case No. I. was, and at these times would keep her bed for over a week. Solid food of any kind would then excite convulsions. It was my privilege to see her during a convulsion. The first thing that attracted notice was the disturbance of respiration; there was a spasmodic catching for breath, a contraction of one or both sterno-mastoids, or the sub-mental muscles. The patient raised her body from the bed in a state of opisthotonos. There was also trismus, like the other case, and choking, from the contraction of the muscles of the larynx and pharynx. Her face was almost livid; there was some marked irregularity of heart-action. These convulsions passed off in about five minutes. During their occurrence there was perfect consciousness. This patient had vomiting, which depended clearly upon sudden central discharges of nerve-force.

Certain features of both these cases show decided trouble in the medulla. The respiratory neuroses depend upon lesions at the nuclei of the pneumogastric. In Case No. I. the hæmoptysis certainly arises from improper innervation of the lungs. The hypoglossal is involved, in one case, and the accessorius in both of them.

The case of convulsive tremor, reported by Dr. Hammond, and the neurosis reported by Toulmouche,[1] and quoted in the "*Clinical Lectures upon Nervous Diseases*," by Drs. Hammond and Cross, are somewhat like those I have just described.

A case presented by Dr. Janeway to the Pathological Society, of this city, presented symptoms during life very much like those of Case I., but after death the heart was found to be enlarged to a great degree.

A class of diseases of the medulla, characterized by gradual failure, are those of a paralytic nature, and differ from the functional diseases, which are characterized for the most part by discharging lesions. Glosso-labio-pharyngeal paralysis is

[1] *Observations de quelques Fonctions involontaires des Appareils de la Locomotion, et de la Préhension. Mémoires de l'Academie Royale de Médecine*, tome II. Paris, 1833.

the most prominent of these. The advance is gradual, there being impairment first of the facial muscles, insensibility of the buccal mucous membrane, drooling of saliva; next, involvement of the tongue, imperfection in speech and deglutition, and finally, when the disease invades the pneumogastric nuclei, death. The duration of the disease is from a few months to two or three years. The intellect is clear throughout.

The paralysis of the facial muscles is interesting because of its completeness. While the eyes and upper muscles retain their expression, the lower part of the face seems covered by a mask.

The muscles retain their susceptibility to electric irritation, and this is a useful fact in diagnosis. Double facial paralysis is characterized by loss of electric sensibility. There are other diseases that may be confounded with it in its early stages; these are general paralysis, and diphtheritic paralysis of the muscles of the soft palate. In the former, there is no paralysis of the tongue, and the defects in articulation are different; in the latter there is no involvement of the muscles of the lips or tongue. Sclerosis may occasionally involve the medulla, and produce symptoms clearly characteristic of loss of function in these nerves I have alluded to.

Charcot[1] gives, among other cases, one that involved the medulla extensively. A patient of his presented, besides the ordinary symptoms of disseminated sclerosis, three months afterward, evidences of invasion of the pneumogastric and hypoglossal nerve-roots. There were dyspnœa and dysphagia. The patient was obliged to eat more slowly; oftentimes the food was regurgitated through the nostrils. Death followed in about six weeks afterward, preceded by asphyxia.

The autopsy revealed the following state of the nervous centres: A section made one centimetre below the protuberance, at the point of origin of the trigeminus, disclosed a point of sclerosis. Other transverse sections were made at

[1] *Leçons sur les Maladies du Système Nerveux*, Paris, 1872–'73. Première partie, p. 234.

the smaller part of the olivary bodies, and a sclerosed patch was discovered. Another patch was seen at the root of the pneumogastric. Examination by the microscope revealed a number of broken nerve-tubes and broken-down cells at the nuclei of the hypoglossal, and traces of irritation in the white substance of Schwann in the pneumogastric fibres. The pharynx and larynx were healthy.

Numerous acute morbid states, the reverse of the above, softening being one of them, may give rise to symptoms that can only be produced by disease of this structure. Oftentimes these central conditions are rapidly fatal, and sometimes instantaneously so, when the destruction by inflammation takes place at the point of the *calamus scriptorius.* Softening very rarely confines itself to this organ, but takes place as an extension of a morbid process from other parts.

Tumors in the substance of the medulla are rare, but when they occur there is pain with gradual loss of those functions I have alluded to. All of the neuroses of the medulla characterized by convulsions are dependent, to a great extent, upon inhibitory causes, and the phenomena of reflex action enter largely into their production and continuance.

In the reflex neuroses I have spoken of, we may assume that there are two factors in the etiology of the disease, and its paroxysms: 1. A seat of primary irritation, usually continued, from which impressions are made upon the centres at which the disease is established. 2. A secondary seat of irritation, from which temporary excitations are sent to provoke nervous discharges after the disease is established.

The disease may arise from only one organ, and subsequent irritation at this point may provoke paroxysms. I may explain by supposing a case of epilepsy, produced by a long-continued uterine irritation.

Occasionally seizures may be incited by disturbances of digestion, or the introduction of irritating substances into the stomach. To illustrate the other form, we will take gastric epilepsy, or will suppose the patient has had some long-continued uterine irritation, and that certain reflections of irrita-

tion have been transmitted to the nervous centres. The irritation attendant upon the menses will produce attacks with organic trouble, seated in the medulla. In hydrophobia and tetanus the reflex excitability is so great, and the nervous centres so ready to be acted upon, that simple blowing upon the surface will often produce sensory impressions carried rapidly to the cord, and marked disturbances are the result.

Ganglion-cells are found in large numbers in the gray matter of the medulla. They are undoubtedly increased in certain neuroses, particularly those of a reflex character. In the latter stages, where the changes in nutrition follow, and after death, when the mechanical violence of the circulating blood, associated with asphyxia, breaks down nervous structure, we will see laceration and disappearance of cells and nerve-tubes.

Duchenne's and Westphal's views in regard to the functions of nerve-cells, although lacking confirmation by the microscope, are certainly attractive. These views, advanced in 1870, assumed that there were three sets of cells—sensory, motor, and trophic—and that they laid in close contact, and were connected. They found that atrophic conditions of the facial muscles and tongue were associated with disappearance of certain cells which Duchenne is pleased to call trophic; that if there had been no atrophy, but simply loss of power, then the disappearance of *motor* cells would follow.

As I have said, this ingenious theory has been doubted, so far, by microscopists, for no difference has been detected in the character of nerve-cells, so far as their quality is concerned.

Post-mortem examination of the bodies of patients who have died of these neuroses have revealed widely-differing appearances. In one instance I was unable to see any thing abnormal in the medulla of a patient who had died with well-marked glosso-labio-pharyngeal paralysis, and several gentlemen, of much greater experience in microscopy than myself, found neither gross nor microscopical changes from the healthy state.

The observations of the various European authorities differ,

but all present certain common features. Fox[1] considers an absolute or partial disappearance of the nerve-tubes, with preservation of the neurilemma at the nerve-roots, to be a constant lesion. Trousseau has seen a granular, fatty deposit in the neurilemma. Wilks[2] found the roots of the hypoglossal and spinal accessory nerves to have undergone atrophy, and become reduced to "little thin gelatinous threads."

In sclerosis, we shall find the characteristic increase of the neuroglia. The general rules for the recognition of well-marked neuroses will enable us, during life, to look for the location of the post-mortem appearances.

I make the distinction between the condition of true sclerosis and the lesion of glosso-labio-pharyngeal paralysis. In the first there is the increase of neuroglia; in the other the neurilemma is left, but there is no increased thickening.

In the convulsive diseases there will undoubtedly be fouud a vascular change. Autopsies of patients that have died of epilepsy have disclosed a marked enlargement of the vessels, and a perivascular exudation.

Hæmorrhage into the medulla is an extremely rare and fatal occurrence. I believe there have been cases recorded where the patients have lived; have been hemiplegic with troubles of deglutition, and certain defects of speech not aphasic in character, continuing after there was improvement in other parts. Some acute convulsive diseases are connected with disorganization of the nervous substance, extravasation and laceration of this organ.

Fatty degeneration will account for many of the peculiar neuroses of this region, but the microscopist should not be misled by the same appearance that may follow the administration of phosphorus, when perhaps this drug may have formed a part of the ante-mortem treatment.

Gross post-mortem appearances have been found in several instances. In one of Van der Kolk's patients, there was marked atrophy of one olivary body, in a patient in whom paralysis of the hypoglossal had been marked before death.

[1] Pathology of Nervous Centres. [2] Ibid.

A few words about the treatment of these conditions would perhaps be *à propos.* Where these neuroses are met with, our first indication, it seems, is to diminish the irritability of the medulla, and improve nutrition. If it be an organic neurosis, there is little to do.

In the functional trouble, ergot has been an admirable remedy, whether we have reason to believe there are organic changes or not. This is particularly true in epilepsy, for here I consider the medulla to be the *point d'attaque.* Used conjointly with the bromides, it is of immense value.

At the same time we must endeavor to find a seat for reflex irritation to start from, and we shall often discover the uterus and stomach to be the offending organs. The diseases dependent upon gradually produced structural changes in the medulla, and characterized by rapid decline, are rarely amenable to treatment.

Phosphorus and its compounds, with the exception of the zincic phosphide, which I believe to have very little virtue, may retard the disease, but never cure it.

Galvanism rarely does good. Sulphur-baths and counter-irritation merely improve the patient's condition for a short time.

Altogether the prognosis of the organic forms of these diseases is bad.

REPARATION OF BRAIN-TISSUE AFTER INJURY.

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Read February 18, 1875.

In February, 1868, I saw, with Dr. Edwin Hutchinson, of Utica, a case of fracture of the cranium, in H. Galli, a boy three years old. He had fallen against a stove, striking the right side of his head. No unconsciousness followed; when we saw him he was sitting on his mother's lap, not complaining, and the side of his face was smeared over with brain-substance. Examination revealed fracture of the anterior portion of the right parietal bone, a piece of which, more than an inch in diameter, had been driven into the brain, standing at right angles with the surface, with one edge still adherent. It had torn through the membranes, and lacerated the brain-substance, a portion of which had been forced through the opening. After laying back the scalp by a V-incision, the tearing up of the brain-convolutions was very apparent. Elevating the bone forced out a quantity of brain-substance. When the edges of the wound were brought together, a small opening remained in the cranium, where an irregular piece had been broken off. For seven days the child continued quite comfortable; a little fever, but pulse not rising above 132; appetite good, and did not complain of pain.

On the seventh day he became restless, head hot, some discharge of thin pus; pulse rose to 140. This condition continued for three days, when the flow of pus ceased and the ragged membranes projected through the opening. The child was dull and somnolent. The wound was carefully explored with a probe, and the projecting membranes clipped

off. This was followed by a small discharge of pus, and on the following morning the pulse fell to 116; heat of head lessened; the boy brighter.

On the eleventh day there were twitchings about the right eye, and the eyeballs were in constant oscillation horizontally.

On the twelfth day there were loss of movement and lowering of temperature in left arm; pulse 112 when awake and 100 when asleep; skin was cool, tongue moist, and urine free; child restless, crying, and at times screaming. A probe was again passed into the wound, and a free discharge of dark-colored pus followed, with some broken-down brain-tissue. Immediately after, Dr. Hutchinson removed a spicula of bone which had been embedded some two inches in the brain, and the child brightened up.

On the fourteenth day, pulse 98, movement of the eyeballs ceased, and slight motion and increase of temperature appeared in the left arm. Quite a free discharge of pus.

On the sixteenth day, movement in arm returned and child bright; flow of pus continued.

On the twentieth day, a small growth protruded through the opening, and, increasing, pressed the piece of bone backward and downward. This mass was again cut away. It was rapidly renewed, however, and was again cut away on the twenty-first, twenty-third, and twenty-fifth days.

On the thirty-first day a protrusion, the size of a pigeon's egg, was cut away.

On the thirty-second day a still larger mass was removed.

On the thirty-third day some hæmorrhage occurred.

On the thirty-seventh day the protruding mass was the size of a large hen's egg.

On the forty-fourth day the mass was tied off, for fear of hæmorrhage.

On the forty-sixth day a protruding mass was again cut away.

On the forty-eighth day a flow of serum commenced, which continued until the fifty-third day, during which over

two pints was discharged. During all this time the child was bright but fretful, took food, pulse ranging from 120 to 144.

On the fifty-eighth day the protruding mass remained stationary, its broad base filling the opening, firm, of a light pink color, and resembling brain-tissue. Child deaf, walks unsteadily.

On the sixty-eighth day, general health improved, child walks alone: all the general symptoms have subsided. The mass has the appearance of brain-convolutions.

On the sixty-ninth day Dr. Hutchinson commenced slight pressure, by a cork pad, held by a rubber band passed around the head, such as is used around packages of letters, and in a few days, without any unpleasant symptoms, the mass passed within the cranium.

On the eighty-eighth day it was covered with membrane.

On the ninety-eighth day the scalp had nearly healed over.

On the one hundred and twelfth day the child was entirely well, but deaf.

In this case, considering the age of the child, the loss of brain-substance was large. During the inflammatory process, while the injured brain-tissue was being discharged, the amount of connective-tissue elements produced and cut away was very great.

Dr. S. Weir Mitchell, in speaking of the pathological results of neuritis after injury to nerves, says there is "an enormous development of connective-tissue elements."

The length of time in reparation of tissue in this case quite corresponds with reparation of nerves after injuries.

The new and final growth was the reformation of brain-matter filling up the space. It was some twenty days in completing its structure, and the convolutional character of the surface was distinctly marked. My impression is, that it would have passed into the cranium *without* the slight pressure used.

If, after the section of a nerve, the upper end should be

renewed by the formation of a button-like growth, and, if reparation is a law of the organism, why should the brain be an exception? Was the brain-tissue reproduced, or the space simply filled with amorphous matter? It was a long time before the reproduction of nerve-tissue was accepted; yet, nerve-fibres were reunited and reproduced in the healing of wounds and fractures, and in cases of destruction of tissue by abscess or ulcerative processes.

This boy is now ten years old, a bright, active lad; is deaf, and is being educated at the Institution for Deaf Mutes in New York City. The case was under the charge of Dr. Edwin Hutchinson, who conducted it with great skill, and whose reported notes I have freely drawn upon.

The second case is that of a man, a soldier, who at the age of forty-one, in September, 1862, was wounded in the head at the battle of Antietam. The ball struck the posterior part of the right parietal bone, crushing it in, leaving an opening in the skull one and a half inch in antero-posterior diameter, and three inches in the vertical line. The ball was embedded in the substance of the brain. Forty-eight pieces of bone were taken from the brain; the bullet was removed ten days after the injury. He was discharged from the service and pensioned in 1863, and entered upon his occupation as a turner in brass, the wound having entirely healed over. For five years he remained in good health, without even suffering from headaches. His general health became impaired in October, 1868, and he became depressed, and finally developed an attack of melancholia.

He was admitted to the asylum at Utica, February 3, 1871, where he remained until February 15, 1872, having then been well about four months. He was discharged recovered, and returned home and to his work. Nine months afterward he began to suffer from pain in the head, especially over the frontal region. He complained of confusion of mind, and asked to be again received at the asylum, dreading a return of melancholia. In April, 1873, this condition continu-

ing, he applied for an order, and came to the asylum alone with the papers of commitment. June 14th, two months after admission, he became profoundly melancholic. He gradually failed, and died July 25, 1873.

On *post mortem*, an elliptical opening in the right parietal bone was found, corresponding to the wound already described.

No attempt at bone-reparation had been made. The dura mater extended over the opening, and was firmly adherent to the scalp. The arachnoid and pia mater were so completely renewed that no trace of the injury could be detected in their structure or by the presence of cicatrices.

The convolutions were fully outlined, and resembled in appearance the other convolutions of the right hemisphere. Vertical sections through these repaired convolutions showed the normal proportion of gray and white matter.

As the pathological result of the attack of insanity, the dura mater was somewhat thickened, and showed signs of recent inflammatory action. The pia mater of a large part of the right hemisphere was opaque, and raised by an effusion of serous fluid, containing lymphoid cells and pus corpuscles.

The whole brain was dry, atrophied more or less, but especially the convolutions of the right side. Each ventricle contained half an ounce of serum. The brain weighed thirty-eight ounces. The walls of the vessels of the convolutions, in certain areas, were distended by crystalline deposits of cholesterine, and structureless, translucent bodies of an albuminous character. Amyloid degeneration was found in the walls of the vessels of the pons Varolii and the medulla. The nerve-cells of the outer layers of the gray substance were contracted and opaque. The fibres of the white substance thicker than usual, and the neuroglia lessened.

In the atrophied gray cortex of the anterior and posterior ascending parietal convolutions of the right hemisphere, the nerve-elements were much diminished in number, in comparison with the corresponding parts of the left hemisphere. This condition was especially marked in regard to the pyramidal

cells of the second of the five layers of the cortex. The connective elements were more dense, fibrillous in their structure, and densely colored by carmine.

In the middle and inferior frontal convolutions, down to the convolutions of the Sylvian fissure and the island of Riel, there was large infiltration of pigment.

In this case, as in the other, the brain-reparation was complete, and the man remained well for five years. The pathological results of the attack of insanity were similar to those ordinarily found.

Theodore Simon (Virchow's "Archives") reports two cases of what he denominates additional brain-growths, where new formations were found superincumbent upon the gray matter of the convolutions. In these new growths the gray and white matter were normal in their relations and proportions. They probably originated from slight injuries.

Pathological history affords a large number of injuries to the brain, with loss of brain-substance and subsequent recovery, though there have been comparatively few cases where they have been followed through life, and the brain examined after death.

Among the most interesting cases is that of Phineas Gage, which occurred in Vermont, September 13, 1848, and is given in detail in the descriptive catalogue of the Warren Anatomical Museum of Boston. The skull is now in the museum. A tamping-iron, a cylindrical iron bar, one and a quarter inch in diameter, three feet and seven inches in length, and weighing thirteen and a quarter pounds, passed through his head while he was tamping a charge for blasting rocks. One end of the bar was square, the whole tapering to a quarter of an inch at the opposite end. It entered "in front of the angle of the lower jaw, upon the left side," by the smaller end, and passed out through the anterior and upper part of the left parietal bone. It traversed "the anterior part of the left hemisphere, and across the corpus callosum and the margin of the right hemisphere, involving the loss of the central part of the left anterior lobe, together with extensive

laceration of the middle lobe, the right central lobe, the falx, and the longitudinal sinus." Here was an immense loss of substance.

In this case, as in the boy Galli, a large fungous growth appeared in the progress of the case, and was cut away; there was also discharge of pus and broken-down brain-tissue.

On the fifty-sixth day he was so far recovered as to be walking about.

On the sixty-second day he walked half a mile.

On the seventy-third day he went to his home, a distance of thirty miles.

On the one hundred and ninth day "the wound was quite closed."

It will be observed that the progress of brain-restoration in these two cases is quite similar.

He lived twelve years, some two years of which he traveled with the bar and exhibited himself—then acted as a hostler. In 1852 he went to South America and drove a stage-coach. In June, 1860, he returned to San Francisco, with impaired health, where he worked on a farm, till he died of convulsions on the 20th of May, 1861.

It is to be regretted that the record is silent in regard to the condition of the brain. The probability is, that the space was so completely filled up as not to attract the attention of those who made the *post mortem* and preserved the skull.

I have seen three cases of attempt at suicide by shooting, where the ball entered the brain and remained there, and where recovery took place. In each case the external wound was *kept open*, and pressure prevented during the progress of reparation.

SIGNIFICANCE OF DISTURBED ACTION AND FUNCTIONAL MURMURS OF THE HEART.

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Read March 18, 1875.

In April, 1868, I had the honor of reading a paper on "Cardiac Murmurs" before the New York County Medical Society, in which my endeavor was to substantiate the true diagnostic sign of mitral regurgitation; and also the significance of intra-ventricular or mitral non-regurgitant murmurs, as were held by my friend the late Dr. Cammann. He had demonstrated, by pathological investigations, that the signs of mitral regurgitation as generally taught—murmurs at the apex beat, blowing, sawing, rasping, etc.—were unreliable, but that the true and invariable sign is a murmur of an entirely different character—a soft murmur, a friction-murmur, such as would naturally be formed by forcing fluids through an aperture, and which is heard behind, between the seventh and eighth vertebræ of the left side, close to their spines; and that, with this sign alone, mitral regurgitation is with certainty diagnosticated. The mechanism of the first sound is evidently the key to a correct diagnosis of a large majority of murmurs, both functional and organic. The theories of the cause of the first sound, according to Bellingham, "may, for convenience' sake, be considered, as the cause is supposed to be extrinsic or intrinsic to the heart. Thus under the first, it has been attributed to the impulse of the apex against the parietes of the chest; under the second head, it has been attributed to muscular contraction—in other words, to the successive shortening of the muscular

fibres of the parietes of the ventricles. This is the oldest theory; it was adopted by Harvey, Haller, Senac, Bichat, and Corvisart. 2. To the sudden tension of the auriculo-ventricular valves. 3. To the friction of the blood against the parietes of the interior of the ventricles, or of the orifices of the large arteries. 4. To the collision of the opposite internal surfaces of the ventricles at the conclusion of the systole. 5. To the sudden elevation of the sigmoid and semilunar valves, caused by the wave of blood transmitted by the ventricles. 6. To the concussion of the blood transmitted by the systole of the left ventricle, with that contained in the aorta; and, lastly, to two or more of the foregoing causes combined."

I chose to consider as worthy of attention only three of the theories in vogue: 1. That of friction of the blood in its motion, within the ventricle, and its passage into the aorta. 2. That of the muscular contraction of the heart itself producing sound-vibrations, as shown by Dr. Wallaston, in 1810; and, 3. That of the vibrations of the mitral valve, caused by its closure and tension, and the forcing and rushing blood; and lastly, that some, recognizing the possibility of each of these three causes mentioned producing sound, have believed that, as the first sound is evidently composite, it is the result of all three. This was the theory held by Dr. Cammann.

As none of these theories seemed to me to agree with all the conditions, and especially with acoustical conditions, I was impressed with the truth that they did not give satisfactory evidence of the cause of the first sound, and that we must direct attention to the heart itself for new proof on this vexed question. We find a peculiar musical-instrument arrangement within the heart, of a drum-like expansion of fibrinous tissue, to which are attached fine, tendinous cords, joining each part of the valve to the wall of the heart, through the intervention of bundles of muscular fibres—columnæ carneæ, or musculi papillares. It seems incredible that such admirable conditions for producing sound-vibra-

tions could have so long been overlooked by the many able observers, as the most probable cause of the first sound.

That the first sound is caused by vibrations of the chordæ tendinæ, connected with the mitral valve in the left heart, and with the tricuspid in the right, set in motion by the swift current of forced blood, is a reasonable postulate. If this doctrine can be proved by pathological evidence of undoubted character, it simplifies our investigation. If plastic lymph be exuded upon the surface of the valve, or upon its edges, gluing them together, and if at the same time the chordæ tendineæ are shortened and thickened by exuded plastic lymph, or glued down upon the valve so as to prevent vibration, then, if the first sound is altered, and all murmurs are abolished, it must be admitted that the proof is sufficient. The following cases are offered as supplying such convincing evidence:

Case I. *July* 6, 1859.—John Martin: is a native of England; educated at Eton; forty-two years old; during the last ten years has been dissipated, and has had syphilis; had rheumatism eight years ago, which kept him in bed two weeks; and has since had frequent rheumatic pains; with these exceptions, has been well until about two years ago, when his appetite failed, and he vomited mornings after taking beer; and his weight declined from one hundred and ninety to one hundred and forty-four pounds. Two days ago, while at his business, there was momentary loss of consciousness without falling, and similar attacks occurred frequently until last night, when they prevented sleep.

Examination.—The pulse grew gradually weaker, until it could no more be felt, and at the same time the respiration would be suspended. The interval was so long, that I looked in his face to see if he were not dead; when, with a full inspiration, and a strong throb of the pulse, both would commence again and continue about fifteen pulse-beats, then cease, and begin again as before. In addition to this were the attacks of "petite mal"— his face would flush slightly, and his eyes stare as if he saw a strange object—this would

scarcely interrupt his conversation, when he would go on again as if nothing had happened. These epileptiform seizures came during the intermissions of the pulse and breathing, as well as at other times.

Auscultation of the chest discovered no fault in respiratory murmurs. There was a slight systolic cardiac murmur, aortic-obstructive. After an intermission of the heart-beat, which agreed in length with the intermission of the pulse, it would begin again with a forcible impulse, which gradually decreased in strength until it ceased to be felt or heard, after which one contraction of the heart could be heard, but without first sound or impulse. The sound of this contraction was peculiar; it was as if no blood was forced into the aorta by ventricular contraction. By careful counting, repeated a number of times, the exact time of the heart's rest was found to be sixteen seconds. The heart seemed to beat in a wild and peculiar manner, as if outside of the pericardium, and the point of impulse varied an inch or an inch and a half.

The next day Dr. T. M. Halstead was called as counsel, the conditions remained unchanged.

8*th.*—Was called at 6 A. M to see the patient, who was supposed to be dying. I was informed that an intermission of extraordinary length had occurred. Respiration and pulsation had ceased, the hands fell by his side, his chin dropped, his head inclined to one side, and his face became livid. His sister, who sat by him, believing him to be dying, called his wife; her outcries awakened him, and after a short time he recovered, and was as he had been before. When I arrived his pulse was 25 in the minute, as it had been from the first, and his state remained unchanged in both signs and symptoms.

Friday, 10*th*, 7 P. M.—Dr. Alonzo Clark was added to the consultation. Dr. Clark found the time of intermission of the pulse to be thirteen seconds; the seizures are a little more violent, and he is nervous. Physical signs the same as before.

11*th.*—Patient has slept during the night. The epilepti-

form seizures ceased at midnight, and the pulse has become regular without intermissions—52 in a minute. After this the patient steadily improved, and one month afterward he walked to Dr. Cammann's office in Fourth Avenue. Dr. Cammann diagnosticated systolic obstructive murmur, with hypertrophy of the heart, but believed the irregular action and peculiar symptoms were owing to functional derangement from indigestion. He became well enough to attend to business until October, 1861, when he was again taken ill. There were anasarca, dyspnœa, and laboring heart and obscure physical signs. He gradually failed, and died on November 26, 1861.

Post mortem on 27th, assisted by Dr. Loomis. Complete adhesion of the pericardium to the heart. There was no free space, but in some parts the adhesions were stronger and apparently older than in others. The heart was largely hypertrophied, but was not weighed. The curtains of the aortic valve were thickened and shortened to incompetency, not holding water. The edges of the mitral valve were glued together, extending into the ventricle like a funnel: complete stenosis. The opening very small, the valve and chordæ were thickened and covered with plastic lymph, white and glistening.

CASE II. (*Substance of Remarks made by* JAMES R. LEAMING, M. D., *before the Pathological Society on the Presentation of a Specimen for a Candidate for Admission.*)—Mrs. B——, twenty-three years of age, native of New York, widow, called Dr. S——, in April, 1869, for advice as to cardiac trouble and swelled feet. The doctor found, on examination, a systolic murmur over the base of the heart, more distinct over the aortic valves, gradually disappearing to the right in the course of the aorta; there was also a diastolic murmur.

Diagnosis.—Aortic obstruction and aortic regurgitation, with hypertrophy of left ventricle. There were also casts in the urine and albumen. She became dropsical, her condition gradually grew worse, and she died in September last.

I saw the case with Dr. S——, in May, and found no dif-

ferent conditions than those already discovered. *There was no mitral murmur of any kind.* The specimens here presented show Bright's small kidney of advanced disease. The heart is hypertrophied mostly in the left ventricle; the aortic valve is thickened at the base of the curtains; shortened to incompetency—so far, agreeing with the diagnosis. But the mitral valve presents the most notable feature. There was no sign of disease of this valve during life, and yet it is damaged in a very peculiar manner. It is thickened by lymph-deposit; its color white, opaque; the edges of the curtain are adherent, and the orifice is narrowed down till it will barely admit the top of the index-finger; and the whole valve extends down into the cavity of the ventricle like a funnel. The chordæ tendineæ were shortened and thickened by lymph-deposits, and the musculi papillares were thickened and lengthened. But every thing was symmetrical, viz., the funnel-like condition of the valve, the hypertrophy of the cardiac walls, of the musculi papillares, and of the columnæ carneæ. With perfect conditions for producing a *mitral direct murmur*, it was absent.

Case III. (*Copied from Reports of the Pathological Society, published in the Medical Record in* 1871.)—Dr. Loomis presented a heart, with the following history, from Dr. Milliken, house-physician of Bellevue: "Henry Clemens, admitted April 11, 1871, aged thirty-two; single; cabinet-maker by occupation; nativity, Switzerland. Patient gives hereditary history of pulmonary phthisis. Had an attack of articular rheumatism when seventeen years of age, from which he made a good recovery. States that neither at that time, nor since, has he experienced any precordial pain, but has noticed that after indulging in tobacco (for he has been an inveterate smoker) he would suffer from palpitation of the heart. He had had a cough, dating some time back, with some expectoration of a pearly white material, which he says he coughs up at night, at which time his cough distresses him most. About two weeks ago, for the first time, he noticed that the sputa were streaked with blood. His cough remained about

the same in character, until one week ago, when he experienced a severe paroxysm of coughing, which was instantly followed by hæmoptysis, which continued for two or three days. Since the occurrence of hæmoptysis, he has had night-sweats, loss of appetite, depreciation of strength, and experienced a feeling of general *malaise*, and inaptitude for any kind of work; he complains also of insomnia and restlessness. His pulse is about 80, regular, but quite feeble; respiration somewhat hurried and easily performed. Heart: action regular, but *quite feeble;* apex-beat on a level with nipple in fifth interspace. Heart-sounds *feeble;* after repeated examinations, no *murmurs could be detected.*"

The record proceeds to say that, while the patient was at dinner, he became suddenly unconscious and fell from his chair, and symptoms of paralysis continued until the 18th, when he died. *Post mortem* showed embolism of middle cerebral artery of left side, with softening of brain-tissue. "Heart, fourteen ounces. Both right and left cavities contain large clot of blood; substance of heart relaxed; stenosis of mitral orifice only admits little finger; some shortening of chordæ tendineæ. The stenosis is due particularly to the thickening, shortening, and adhesion, of the chordæ tendineæ of the valve. The anterior portion of valve forms a bony mass, occluding that portion of the orifice. On the auricular aspect, the surface of the valve is ulcerated, the bony matter laid bare, and soft, reddish vegetations on the free border of the valve and upon the ulcerated surface. Pulmonary and tricuspid valves normal; little thickening at base of aorta."

Dr. Loomis remarked, "The case is of special interest, because with this marked stenosis no murmurs existed;" and Dr. Flint remarked that "the absence of murmurs might be accounted for—1. On account of rigidity of the valve not allowing a vibration; and, 2. The smoothness of the ventricular surface of the valve."

The first case is full of instruction in its facts as regards functional disturbances of the heart and proof as to the mechanism of the first sound. The long period of rest, six-

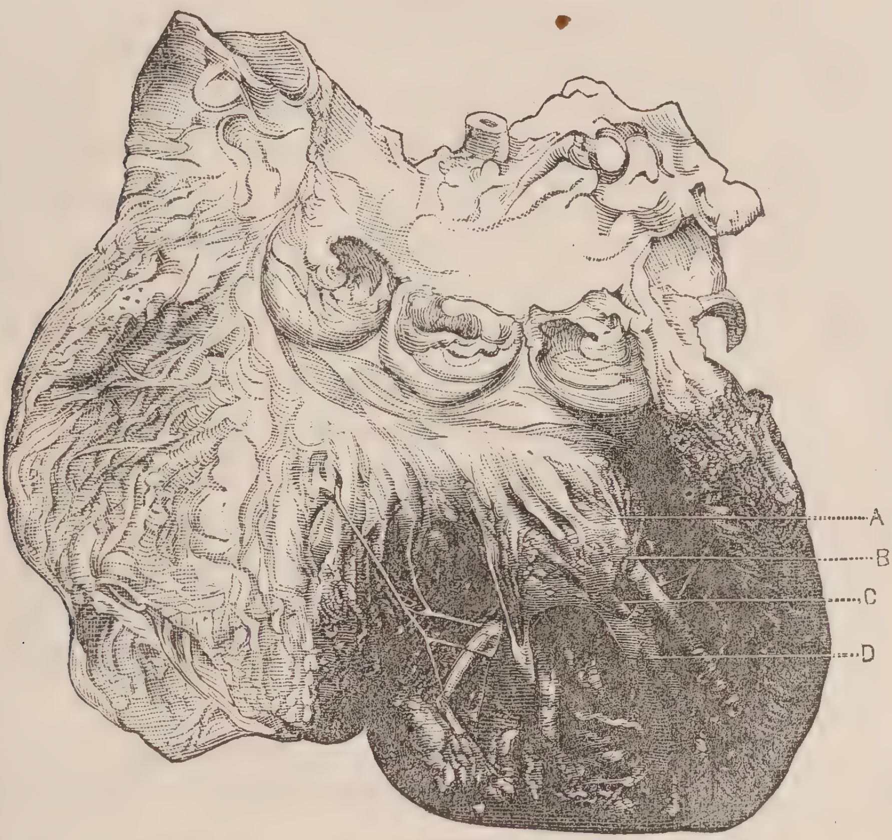
teen seconds, is worthy of our earnest attention. Observers who have watched the action of the heart in ectopia in an infant, as Cruveilhier, Bryan, and others, as well as when the heart has been exposed in experiments upon animals, tell us that the contractions of the auricles continue regularly, although the ventricles may be in a state of rest. And in this case no doubt they did so, notwithstanding that there was no first sound, no impulse-beat, and consequently no contraction of the ventricles. The importance of this fact cannot be over-estimated, because it invalidates much of the theory in vogue in regard to the causation of murmurs. It proves that the auricular systole may take place regularly, even when the auriculo-ventricular opening is very much contracted in stenosis of the mitral valve, without producing sound. Carefully listening under favorable circumstances after the last impulse-beat and first sound, one contraction, presumably that of the ventricle, could be heard, without any vocal element of first sound, and was then followed by the long interval of silence, in which no contraction or sound of any kind could be heard.

The second case is a demonstration of the cause and mechanism of the first sound. *There was no mitral murmur.* With stenosis of the mitral valve, if the chordæ tendineæ had not been rendered incapable of sound-vibrations, by being plastered over with fibrinous deposit, there would have been a murmur, such as is usually heard in stenosis where the chordæ are free and uncovered. The first sound, and all murmurs connected with it, disappearing when the mitral valve and chordæ tendineæ are rendered incapable of sound-vibrations, is as convincing proof of their cause, as is the experiment of hooking up a curtain of the aortic valve proof as to the cause of the second sound.

The second and third cases are confirmatory proof, by different observers, that the cause and mechanism of the first sound and the murmurs connected with it, depend upon the state and condition of the mitral valve and its chordæ tendineæ. In the second case there was no physical sign of dis-

ease of this valve during life, and yet it was found after death to be damaged in a very peculiar manner—thickened by lymph-deposits, opaque, its color white, the edges of the curtains adherent, the orifice narrowed down, barely admitting the tip of the index-finger, and the whole valve extending down into the cavity of the ventricle fixed and like a funnel. The chordæ tendineæ were shortened and thickened, some of them glued to the valve, and the musculi papillares thickened and lengthened, as the specimen which I now present to you

Fig. 1.



A, Stenosis of the mitral valve; *B*, Chordæ tendineæ glued to the valve; *C*, Chordæ tendineæ shortened and thickened; *D*, Hypertrophied and lengthened musculæ papillaria.

demonstrates. This case, during several months, was under the observation of the late Dr. Sprague, a careful and competent auscultator.

The third case, which is reported in the Transactions of the New York Pathological Society, is also confirmatory proof: "In the morbid specimen there was stenosis of mitral

orifice—only admits little finger—some shortening of chordæ tendineæ. The stenosis is due particularly to the thickening, shortening, and adhesion of the chordæ tendineæ of the valve." During life, "heart-sounds feeble; after repeated examinations no murmurs could be detected. Could the proof be more conclusive?

The following experiments by Dr. Halford, quoted in the *British and Foreign Medico-Chirurgical Review*, April, 1860, is singular proof of the physiological cause of the first sound: "My proceedings were as follows: large dogs were obtained, and as in my preceding experiments (the animals being under the influence of chloroform), the heart was exposed and the circulation kept up by artificial respiration. A stethoscope being applied to the organ, the sounds were distinctly heard. The superior and inferior venæ cavæ were now compressed with bull-dog forceps, and the pulmonary veins by the finger and thumb; the heart continuing its action, a stethoscope was again applied, and neither first nor second sound was heard. After a short space of time, the veins were allowed to pour their contents into both sides of the heart, and both sounds were instantly reproduced. The veins being again compressed, all sound was extinguished, notwithstanding that the heart contracted vigorously. Blood was let in, and both sounds were restored. I have thus frequently interrogated the same heart for upward of an hour, and always with the like result."

The reviewer remarks: "There is an interesting circumstance which took place at one of Dr. Halford's experiments, which appears to us of great importance. It shows that when only a small quantity of blood finds its way into the ventricles, the first sound is still produced. The cavæ and pulmonary veins having been compressed, Mr. Lane, at whose request the experiment was performed, listened to the heart during its contraction, and said he heard the first sound indistinctly, not so clearly as before the compression. On examination it was found that the vena azygos entered the right auricle by an independent opening, and was not se-

cured: the vessel was compressed with the others, the heart contracted, no sound was heard."

This experiment proves that the contractions of muscle of the heart give out no sound which may be an element of the first sound; for without blood moving through the heart it was silent. The remaining elements, friction of the blood against the heart-wall and through the aortic orifice, and vibrations of the chordæ tendineæ and mitral valve, must give answer to the question. When there was no blood forced there was no sound; and we have just shown, by pathological specimen, that when the chordæ tendineæ were rendered incapable of vibration, there was also neither sound nor murmurs. Consequently, the first sound and murmurs must be the result of chord and valve vibrations set in motion by the rushing blood. The blood is the bow applied to the strings to give vibrating sounds; and murmurs are sounds of individual chord-vibrations not in unison.

One of the points I endeavored to establish in 1868 was, that the presystolic murmur, called also the auricular-systolic and the mitral direct, is one of the intra-ventricular murmurs, caused by vibrations of chordæ tendineæ subjected to irregular tension, and not by blood being forced through the contracted opening of the mitral valve in stenosis. Although frequently connected with that pathological condition, it is yet oftener an accompaniment of change of the mitral valve without stenosis. My argument was, as Dr. Cammann contended, that the auricle was too feeble a power to force blood through the contracted opening of the diseased valve, so as to cause sound which may be heard through the chest-wall, even if empty, much less so when the ventricle is filled with blood; and, lastly, I maintained that the murmur does not agree in length with the time of contraction of the auricle. According to the best authorities, the contraction of the auricle is instantaneous, while the murmur is of considerable length.[1] If contraction of the auricle could cause the mur-

[1] Harvey, Lower, Bellingham.

mur, the two ought to agree in time. According to Bellingham, "the systole of the auricle is a quick, short, sudden motion." Lower says, "Its rapidity equals the explosion of gunpowder, and immediately precedes the ventricular systole, the one motion appearing to be propagated by the other."

Marey assigned to it two-tenths of the time of the heart-beat, which is probably ten times longer than the reality, and much less than the time of the so-called presystolic murmur. This murmur, too, has none of the qualities of sound which should be produced by blood forced through a narrowed opening in the valve. But all argument becomes unnecessary in presence of the foregoing pathological facts and clinical history. Dr. Frank Donaldson, Professor of Physiology and Hygiene, and Clinical Professor of Diseases of the Chest and Throat, University of Maryland, in a paper read before the Medical and Chirurgical Faculty of Maryland, annual session, April, 1874, on "Significance of the Presystolic Murmur," relates the following cases, with remarks:

"Some years ago (in 1867), a case came under my observation, which made me question the explanation which I had adopted on the authority of Barth, Roger, Walshe, and Flint, of the sound which was described first by Fauvel, in 1843, and then by Grisolle, as the presystolic murmur, afterward by Dr. Gairdner, of Edinburgh, as the auricular-systolic murmur, and by Dr. Austin Flint as the mitral direct murmur.

"These authorities claimed that this sound was heard just *preceding* the ventricular contraction, and was caused by the systole of the auricle forcing the blood into the ventricle, through a diseased and contracted auriculo-ventricular orifice.

"The case was of a man sixty-four years of age, of grossly intemperate habits, who came to the Baltimore Infirmary with symptoms of advanced heart-disease—great dyspnœa, a small, contracted pulse, heart much hypertrophied, with a murmur of a rasping character, heard loudest between the

second and third ribs at the base, not extending up the carotids, but down toward the base, and completely obliterating the second sound of the heart. The murmur was audible after the apex-beat and the systole of the ventricle, and was followed by the pause of the heart. The first sound of the heart was normal. The diagnosis seemed clear and unmistakable, and was recorded as insufficiency of the aortic orifice, by means of which the arterial blood was forced back into the left ventricle.

"The *post mortem* showed atheromatous degeneration in the aorta above the semi-lunar valves extending to the sacs of Valsalva, and causing adhesion of one of the semi-lunar pouches of the aortic orifice to the wall, so bending it down that that portion of the orifice was unprotected. The second sound could not be produced, and the insufficiency of the valve was evident.

"Thus far the diagnosis was correct, but on examining the mitral orifice we found, to our surprise, that it was reduced by thickening at its base to about the size of one-quarter of an inch in diameter. Yet, during life, there was no abnormal sound preceding or during the ventricular systole. With such a contraction of the left auriculo-ventricular orifice, ought we not to have had a decided presystolic murmur? The whole heart, auricle and ventricle, was enlarged and increased in force, and yet there was no murmur produced from the passage of the blood through an orifice so reduced in size! I could not help questioning the received opinion as to the significance of the so-called mitral murmur. As it is a physical sound, heard at a particular period of the heart's action, the physical cause which was said to produce it being present, it ought to have been heard, but it was not.

"Hope, as far back as 1842, reports a case where the mitral orifice was so contracted that it would only admit the little finger, yet there was no murmur during life, preceding the first sound. In his report he adds: 'I have frequently known a contraction of the mitral orifice to the size of only two or three lines, to occasion little or no murmur.' Dr.

Stokes, in his work on 'Diseases of Heart and Aorta,' relates two cases of extreme contraction of the mitral orifice found after death, but where, during life, there had been no murmur audible even to his practised ear.

"Dr. Waters. His first case was where he heard a loud systolic as well as a presystolic murmur. At the autopsy there were found insufficiency and slight contraction of the mitral orifice. In his second case there was no presystolic murmur whatever, although the autopsy showed a constricted mitral orifice only admitting the tip of the index-finger. Next follow the details of four cases of extreme contraction of the mitral orifice, where, during life, there was no presystolic murmur audible. He candidly adds: 'I have given you instances sufficient to prove that great constriction of the mitral orifice may exist without there being any murmur produced by the passage of the blood from the auricle into the ventricle, and therefore that you must not look for a mitral-diastolic or presystolic as a constant sign of obstructive mitral disease. My belief is, that this murmur is far more frequently absent than present, even when there is great obstruction at the mitral orifice.' Dr. Waters accounts for the presence or absence of this murmur, as depending on the greater or less vigor with which the auricle contracts."

Dr. Donaldson sums up his relation of cases and remarks: "Thus we have eleven cases of the lesion without the murmur, and three cases of murmur without the lesion" (quoting the latter from Dr. Flint).

The diagnostic sign of mitral regurgitation, which has been and is still taught, is a harsh, blowing, sawing, or filing murmur, heard during the systole at the apex-beat. Upon the accepted authority of this murmur, which is so often met with, the great frequency of mitral insufficiency has come to be considered as incontrovertibly established.

The cases we have already related are proof that these murmurs are not heard when the chordæ tendineæ and valve are rendered unfit for sound-vibrations. J. S. Bristow, M. D., London, F. R. C. P., Physician to St. Thomas's Hospital, in

an article on "Mitral Regurgitation, arising independently of Organic Disease of the Mitral Valve," in the July number of the *British and Foreign Medico-Chirurgical Review* of 1861, gives six cases, with introductory remarks. With your permission I will read some of his arguments and quote points in the cases, for the purpose of showing that instead of proving that regurgitation may take place through the mitral valve without disease, as he imagines, they in reality disprove the theory in vogue, and confirm the doctrine of chordæ tendineæ vibrations as cause of the first sound.

Dr. Bristow remarks: "It may almost be regarded as an axiom in medicine, that the presence of a systolic apex-murmur is positive proof of regurgitation through the mitral orifice. I have not hesitated to adopt it in reference to the cases already detailed." The following are quotations from his cases:

Case I.—There was a distinct systolic murmur audible at the apex of the heart.

Post mortem.—The aortic and mitral valves were perfectly natural.

Case II.—There was an increased area of dullness in the cardiac region, and a systolic bruit loudest at the apex of the heart.

Post mortem.—The muscular tissue and the valves appeared perfectly healthy.

Case III.—The impulse was diffused and heaving, but not very strong. A systolic murmur was detected at the apex of the heart.

Post mortem.—All the valves were healthy-looking.

Case IV.—First sound at the apex was flapping and prolonged.

Post mortem.—The valves were perfectly healthy in texture.

Case V.—The cardiac dullness was enlarged, and a systolic murmur was audible with the heart's action, most distinct at a point an inch below, and internal to the left nipple.

Post mortem.—All the valves appeared perfectly healthy.

Case VI.—There was a distinct but not very loud systolic murmur, loudest in the usual situation of the apex of the heart.

Post mortem.—The aortic and mitral valves were perfectly healthy-looking, and doubtless quite competent.

A tabular arrangement like the following, in classifying murmurs acoustically, may be useful :

Valvular (all organic).	Aortic obstructive systolic. Aortic regurgitant diastolic. Mitral regurgitant systolic.
Intra-ventricular (more or less functional).	Organic functional. Inorganic functional.

These two great divisions are made in accordance with their acoustic differences. The sound in valvular murmurs is a friction-murmur, that of blood forced through an aperture. The intra-ventricular murmurs are mostly and distinctly chord-vibrations. The contraction of the muscular walls of the heart and its fleshy columns, the friction of rushing blood among the chordæ tendineæ and against the tense mitral valve, being the occasion of sound-vibrations, but is not the mechanism of the sound itself. As great difference exists between these murmurs as between that of a whisper and that of the voice. The obstructive systolic aortic may be modified by irregular calcifications in the aortic valves, extending into the column of forced rushing blood. In this way a harsher character may be given to the murmur, or it may even become musical. Vegetations also attached to the orifice or valve may be thrown into vibrations in the column of blood, and produce a musical murmur, but these are rare, mere possibilities. When musical murmurs occur they are almost always, if not always, vibrations of the chordæ tendineæ, some of which are under extraordinary tension.

These sounds or murmurs may be illustrated by a stringed musical instrument. Every degree in quality of murmur or sound from the softest blowing, up to the harshest, sawing, rasping, filing, or when the vibrations become sufficiently rapid and regular, into musical sounds. The use of the term

"bellows sound" by Laennec was unfortunate as applied to the murmurs of the heart, and much of the misunderstanding of murmurs and their mechanism is due to it. It is true that it describes the friction-murmur of blood forced through an aperture as in aortic regurgitation. It is like the sound of the air forced through the bellows; but the bellows-sound is not so like the friction-murmur, of blood forced through an aperture, as is fluid forced through an elastic syringe, in which some obstruction is created by pressure upon the tube. But, to imitate the murmur exactly, a fissure should be made in the bulb of the syringe, and then compressing it with force, the fluid escaping will give the exact sound. The only friction-sounds in cardiac murmurs proper are where the blood is forced through apertures or past obstructions; it is heard at the aortic orifice when there is obstruction, as by lymph-deposits upon the valve. It is at first uncomplicated, the simple gushing sound. But in time the obstruction causes hypertrophy of the left ventricle, which, having taken place, irregular tension of the chordæ tendineæ is the result, and vibrations out of unison with the first sound are carried with the current of blood, and both occurring in the systole, are mixed together and form what is called the blowing murmur.

It is now a sound of mixed elements, friction of blood against a solid, and vibration of strings under irregular tension. In order to have an intelligent understanding of these murmurs, we must analyze them and separate the sources of sound. We are assisted in this by localizing the sources. The blowing, sawing, filing, rasping sounds have their origin and cause within the ventricle; they are intra-ventricular. Dr. Cammann called them mitral-non-regurgitant. They are heard over the base of the heart, but always with greatest intensity at the apex-beat.

Friction-sounds are heard best over the orifices or in the direction of the vibrating column of blood. The aortic systolic obstructive murmur is heard over the aortic valves, and in the course of the column of blood. The regurgitant aortic diastolic murmur is heard over the aortic orifice, and to

the left and toward the apex-beat. The mitral aortic-regurgitant is heard behind on the left side near the spine. In this direction the blood is forced in regurgitation through the mitral valve; impinging first against the auricular wall, lying against the œsophagus, and aorta, and intervertebral substance, it is conducted directly into the ear, giving the sensation of being shot into it.

It may be heard a short distance from this point conveyed through the chest-wall. It may be heard in front, at the apex-beat, by conduction through the substance of the heart, when there are no intra-ventricular murmurs to destroy it or take its place. The discovery of this absolute sign of mitral regurgitation belongs to Dr. Cammann, and his last professional thought was given to its consideration. It is one of the most certain of cardiac signs. This characteristic murmur, heard in the situation he has pointed out, is an unfailing sign of mitral regurgitation. It had been my opinion that this characteristic murmur was never heard in front at the apex-beat—as it certainly is not when the valve is diseased, and the loud intra-ventricular murmur drowns and supplants it.

But the following case shows that it may be heard both behind and before in congenital mitral insufficiency, without hypertrophy of the heart and without lymph-deposits upon the valve.

CASE VII. (*December* 12, 1870).—W. S. R., New York, aged twenty-two; mason, living in Yorkville; is a fireman temporarily, and was a member of the old department. Has never been sick, except with chills and fever. Sent for examination by Dr. Charles McMillan, surgeon of the department. There is a systolic murmur at the apex-beat accompanying the first sound; it is a soft, gushing murmur, and can be heard in the chest-wall more to the left than to the right side. It is heard also with directness and greater intensity between the seventh and eighth vertebræ, left side behind, near the spine. The murmur is shot into the ear when placed over this point. It can be heard some distance to the left, conveyed

in the chest-wall. It can also be heard over some portions of the right lung posteriorly, at the inner angle of the scapula; also at the lower angle, being a faintly-conveyed sound.

One year after, examined him again. Signs unchanged. This murmur has the same quality in front as behind. It has none of the vocal element of apex-beat murmurs, usually described as diagnostic of mitral regurgitant murmurs. Yet I have no doubt that this murmur is caused by mitral insufficiency, which is congenital, without hypertrophy of the heart, and without disease of the mitral valve.

A great majority of cardiac murmurs, even of those accompanying organic disease of the heart, are in a manner functional. That is, the murmurs are not organic in the same sense that the valvular murmurs are; which are organic murmurs because the structural change in the valve is part of the mechanism of the murmur. Intra-ventricular murmurs, even when the result of structural change in the heart, may be considered functional, inasmuch as that they have their mechanism in vibrations of the chordæ tendineæ, which are themselves unchanged by any diseased action, but simply vibrate, giving out sound of high or low pitch, soft or harsh, feeble or loud, according to the degree of tension of the individual strings, and the force of the heart's contraction. The cause of irregular contraction of the heart-muscles may be from disturbed nerve power, as well as from organic change.

Functional murmurs proper may occur in the healthy heart, are transient, passing away with the subsidence of the cause, which may be anæmia, hyperæmia, sympathy with brain-disease, stomach, liver, or it may be from disorder of the nervous system, the influence of tobacco, coffee, tea, or any narcotic or stimulant having influence upon the organic life of the body, of which the heart is the centre and citadel.

Functional murmurs proper do not signify danger of sudden death, but nothing more alarms patients than disturbed action of the heart. When the heart seems to stop, and then to turn over and thump against the chest-wall, the

sensation is not a pleasant one, even to a medical philosopher. It is no wonder that it creates intense alarm in the lay patient, especially if accompanied by prolonged palpitation or faintings.

These conditions may be the forerunner of softening, or fatty degeneration, but they signify always that there is over-distention of the portal system, intermission of the heart-beat and pulse, may be present for years, and be merely the result of functional disturbance from chronic indigestion.

Intermissions of the pulse have been laid down in books as signs of heart-disease. Life-insurance companies, in printed forms, make it the duty of examiners to reject as unsafe those who have intermittent pulse. It is possible that this rule militates against the interest of the companies, and it certainly is a source of great alarm to the rejected applicant.

The sign, of itself, is no proof of heart-disease, but is proof of indigestion. It is true, cardiac disease is frequently a cause of indigestion, and thus, secondarily, the cause of irregular pulse. But a confirmed dyspeptic is usually a safe life, for he is not likely to commit indiscretions in diet, as he is continually warned to desist by functional disturbances. Proper medication will generally relieve intermittent pulse, even in advanced cases of cardiac disease.

A sedative dose of calomel will frequently set it right at once, and the intermissions will disappear.

The late Dr. Samuel Henry Dickson stated that, during the first hours of sleep, children have intermittent pulse, which will disappear when they are awakened. This is true, especially with those children who are allowed over-stimulating food, but, as the night passes on, and the food becomes digested, the intermissions cease. In the adult, the occasion of a wine-dinner, with tobacco, is often followed by intermittent pulse, especially during sleep, when the circulation is sluggish.

The cause of the rhythmic movements of the heart is debatable ground. That it is within the heart itself can scarcely

be questioned, for, when the heart of some animals is dissevered from all connections, and taken from the body, it may go on performing its rhythmical movements. Still, the quality and quantity of blood influence them in an unmistakable manner. The fact that shutting off supply of blood to the structure of the heart will arrest its contractions, was shown in 1842 by Mr. Erichsen. Dr. Brown-Séquard has attempted to explain the motion to be due to the carbonic acid present in the venous blood, and Dr. Radcliffe has also given a similar explanation.

The experiments of Dr. Paget show that the power causing rhythmical motion does not reside in all parts of the heart alike; that, in fact—

"If, for example, the cut-out heart (of any of the amphibia) be divided into two pieces, one comprising the auricles and the base of the ventricle, the other comprising the rest of the ventricle, the former will continue to act rhythmically, the latter will cease to do so, and no rhythmic action can be by any means excited in it. The piece of ventricle does not lose its power of motion, for if it be in any way stimulated, it contracts vigorously, but it never contracts without such an external stimulus, and when stimulated it never contracts more than once for each stimulus.

"Other sections of the heart, and experiments of other kinds, would show that the cause of the rhythmic action of the ventricle, and probably also of the auricles, so long as they are associated with it, and not with the venous trunks, is something in and near the boundary ring between the auricles and ventricles; for what remains connected with this ring, or grew with a part of it, in a longitudinally bisected heart, retains its rhythm, and what is disconnected from it loses its rhythm."

If we take a merely material view of the subject, no doubt we have arrived at the solution as nearly as we ever will. But is it useless or absurd to look further? The experiments of the great Harvey with the egg of the hen show that active life remains inchoate in the punctum saliens or

germinal spot until warmed into active life. This principle came into the egg organization at the time of its fecundation. Its first life-motion is rhythmical movement of particles before any portion of the heart's structure can be seen. The little red point appears and disappears rhythmically, and thus the *principle* builds its house, the auricle being its first chamber. The very nature of this principle is rhythmical. Its special home is in the ganglionic nervous system, but it pervades the whole body; wherever there is nerve-fibre accompanying the smallest capillary—the vaso-motor—it is present. Aberration from its normal life-action is disease; and influences, both outside and inside of the body, make impressions upon this life, helping to determine the character of the disease. Medicines act upon it, but their *modus operandi* is a sealed mystery. That they are purgative, emetic, stimulant, sedative, or alterative, we only know the fact. The heart, supplied with about three hundred ganglia, is the centre and citadel of this life, and its abnormal or disturbed action is sometimes mysterious evidence of both intrinsic and extrinsic disease.

Acoustic properties of the chest have not been dwelt upon as their importance demands. The diagnosis of murmurs within the chest is facilitated, or otherwise, according to its conditions as an acoustic chamber. The difficulty of hearing signs in the chest of a hunchback is recognized; it is also a well-known fact that, as the heart enlarges, the murmurs grow weaker, so that those which had been once easily detected become feeble, or disappear altogether. Still, they have been accounted for, it seems to me, upon every other principle than the true one.

In Dr. Cammann's last illness, by his request, I was called to examine him. After he had explained to me that I would find obstructive and regurgitant murmurs, of which he had been long cognizant, and of which he explained the cause and origin, and of their gradual increase, I found that I could but just hear the soft, feeble murmurs of aortic obstruction and regurgitation, but intra-ventricular murmurs were not heard. I told the doctor that the regurgitant murmur which he had

emphasized in relating the case, was slight: "Yes," he said, "it is but a chink." Dr. Peugnet told me that when he examined him at the beginning of his illness, the murmurs were loud and easily heard. I felt mortified that my ear had failed me, as I supposed, caused by a long ride in the cold, in an open carriage. The doctor had circumscribed pleuritis with effusion and pneumonia. In time the effusion was absorbed, and then the murmurs at the apex-beat were easily heard.

Another case, of which I have no notes, in which I failed to make out a murmur where it should have been heard, and which afterward returned, as the inter-current pneumonia, became convalescent, also annoyed me, and again I blamed my ear. Not long afterward I saw in the London *Medical Times and Gazette*, or in the London *Lancet*, the question, "Why do cardiac murmurs disappear during pneumonia or pleurisy?" I felt at once that the cause of my not hearing the murmurs more plainly in Dr. Cammann's case, as well as in that of this other patient, was because they were obscured by some cause I then did not know.

Other cases of cardiac murmurs disappearing or becoming obscured during the presence of pneumonia or pleuritis, led me to believe that it was in accordance with physical law. A patient with pleuritic effusion was sent to me by Dr. Otis for examination. I knew from a previous auscultation that he had aortic obstructive and aortic regurgitant murmurs. At this time, however, they could not be heard. I wrote to Dr. Otis, stating these facts, and predicting that when the effusion was absorbed these murmurs would again return, which proved to be the case.

On August 27, 1864, I saw Miss Hall, matron of the Home for Soldiers' Children, in Fifty-seventh Street near Eighth Avenue, with Drs. Charles McMillan, J. L. Smith, and E. Krakowizer. There were no heart-murmurs, but as all the rational signs of cardiac disease, with increased area of dullness under percussion, signified hypertrophy, it was suggested that we should examine her for pneumonia, and, upon raising

her up and listening behind, it was clearly made out. I then predicted that, when the pneumonia was well, we would be able to diagnosticate her cardiac disease. This was afterward done, and Dr. J. L. Smith took notes of the examination, and upon her death, some months afterward, was able to verify the diagnosis. He presented the heart, with history, to the Pathological Societ y, and a committee was appointe to examine into the facts concerning the disappearance of heart-murmurs during the presence of pneumonia and pleuritis, and to report. If my memory serves me, the committee reported in substance, in the summer of 1865, that in some cases observed in Bellevue Hospital, murmurs grew feeble or disappeared on the advent of pneumonia or pleurisy, and that it was their opinion this phenomenon was owing to the feebleness of the heart and its frequency, for in the cases noticed the pulse was 120 or more in a minute.

These reasons I had myself considered and rejected, for at the same time that Miss Hall was ill, I had another patient, O. B. H——, who had had for years a double murmur, which, when attacked with pneumonia, disappeared. His pulse ordinarily was about 50 in a minute, but during the pneumonia it rose as high as 80, but no higher. Drs. Chas. McMillan and J. L. Smith were also both cognizant of the facts as narrated. The philosophical explanation of these phenomena occurred to me during the winter of 1864–'65, with the following proof and illustration: The chest is a musical chamber, and may be represented by a violin. When the instrument is tuned and in order, its acoustic qualities may be considered as perfect. If a watch or music-box be placed within the violin, hanging from its roof, auscultation will reveal the slightest jar or noise made by the works of the watch, or bring out with distinctness the low tones of the music-box. But, if, while the ear or stethoscope is still placed upon the violin, water or sand be poured into its chamber, the sounds of the box or watch will grow feeble or disappear. The low notes of the music-box disappear entirely, as also does any jarring of the wheels of the watch. These phenomena are invariable

because they are the result of acoustic law. The application of physical law to art is to render it scientific, and scientific medicine is the immediate professional want of our time. If acoustic law is applied to auscultation in physical diagnosis, it will remove it from the domain of doubt or uncertainty, just so far as its principles are intelligently applied.

THE PATHOLOGY AND ETIOLOGY OF PULMONARY PHTHISIS IN RELATION TO ITS PREVENTION AND EARLY ARREST.

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The maladies which have always been the chief enemies of man are the contagious diseases, continued fevers, and pulmonary phthisis. In all ages they have been subjects of constant observation and thoughtful study.

But following Bayle,[1] Laennec,[2] and Louis,[3] a general belief in the specific nature of phthisis seems, at least for a time, to have ended controversy as to its etiology, and to have discouraged the question of its prevention and early arrest.

Bayle and Laennec were victims of the disease they had scientifically defined. Public records and individual observation testify to its preëminence as a destroyer of human life. "It is the chronic plague of the prime of life."[4] In many of the older and populous communities of this country, the mortality from phthisis is one-fifth of deaths from all causes.[5]

Yet there are facts which warrant the belief that phthisis has definite predisposing and exciting causes. To ascertain and diminish these causes is a duty in this age of sanitary science.

[1] "Recherche sur la Phthisie Pulmonaire," 1810.

[2] "Traité de l'Auscultation Médiate," etc., 1819.

[3] "Recherches Anatomico-pathologiques sur la Phthisie," 1825.

[4] "Lectures on Public Health," Dr. Guy, London, 1870.

[5] United States Census, 1870, Washington, 1872.

Pathology.—The history of phthisis is that of tubercle with associated or secondary lesions.

The gross appearances of yellow tubercle have been known for centuries.

Gray or miliary tubercle was first described by Stark,[1] in 1785, by Baillie,[2] in 1794, and more fully by Bayle,[3] in 1810. Tubercle, in these two forms, and the changes through which it passed, has been considered the pathology of pulmonary phthisis. Laennec, in 1819, considered it always a specific disease. Louis accepted the pathology of Bayle and Laennec, and devoted seven years to close observation of the clinical history of the disease. He may be said to be the great advocate of the tubercular nature and unity of phthisis. His views are quite generally held, in this country, at the present day, though elsewhere eradicated by the steady advances of pathology. But independent investigators in France, and especially in Germany, while recognizing the contributions of Laennec and Louis to the diagnosis and symptomatology of phthisis, doubted their views on pathology. Andral,[4] in 1836, said phthisis might occur in persons not predisposed, and in good health, from simple inflammatory processes. Carswell,[5] in 1838, said gray need not precede opaque tubercle. Addison,[6] in 1845, described crude tubercle as a degenerated inflammatory product. The analysis by Preuss[7] proved yellow tubercle to be rich in caseine. Sherer and Lehmann found in different specimens variable quantities of fat. The study of inflammation discovered that fatty metamorphosis is the fate of unabsorbed, unorganized, exudation matter. The charac-

[1] "Handbuch der Allgemeinen Pathologie," Uhle and Wagner, p. 479, 1872.

[2] "Morbid Anatomy," Matthew Baillie, London, 1794.

[3] *Op. cit.*

[4] "Cours de Pathologie Interne," Paris, 1836.

[5] Sir Robert Carswell, "Illustrations of Elementary Forms of Disease," 1838.

[6] "Transactions of Provincial Medical and Surgical Association," 1845.

[7] "Chemical Analysis of Tubercle," by Preuss, Peaslee's "Histology," 1858.

teristics of crude tubercle under the microscope and by chemical analysis were those of metamorphosed inflammatory products. Reinhart,[1] in 1850, advocated their identity. Lebert,[2] in 1851, termed crude tubercle inspissated pus.

Hence the conclusion that "crude tubercle" may occur without the intervention of gray tubercle, a metamorphosis of catarrhal and pneumonic products,[3] and a classification of pulmonary phthisis into—

1. Tubercular } forms.
2. Simple inflammatory }

Still, the indoctrinated faith in the specific nature of phthisis held its ground and endeavored by various theories to connect caseous products in the lung with true tubercular dyscrasia. The *post-mortem* frequence of caseous masses and infrequence of gray tubercle could not be denied. But gray tubercle was defined to be "nascent," "germinal," transitory in office and duration, tending to retrograde at the very beginning of its existence. On the periphery of caseous masses, and in their immediate vicinity, miliary tubercles, some undergoing degeneration, were often found. Caseous matter was therefore asserted to be a devitalized retrograde state of preëxisting gray tubercle. Tubercle corpuscle was designated the product of "specific inflammation," an exudation corpuscle modified by tubercular taint, unfitted for organization and undergoing caseation. But Virchow has established the fact that caseation is essentially the result of the too close crowding of cellular elements, a natural and frequent result of inflammatory infiltration. Hence a large proportion of all cases of pulmonary phthisis are of non-tubercular origin, the sequelæ of inflammatory thoracic diseases.

The gray tubercle was still considered a neoplasm, the product of tubercular dyscrasia, a structure predetermined by the specific vice of the blood.

[1] "Ann. d. Berl. Char.," 1850, p. 362.

[2] "Traité des Mal. Scroph. et Tuberc." ("Lehrb. d. Scrophel- und Tuberkelkrankh.," 1851).

[3] Early edition of Walshe on "Diseases of Lungs."

Piorry said, in 1860, "Pulmonary phthisis is a combination of multifarious, variable phenomena, and not a morbid *unity*."[1]

Niemeyer,[2] though not an originator of the more recent views of the pathology of phthisis, as a systematic author on medicine, has been largely instrumental in establishing their recognition. To the American and English physician who considered "tubercularization" a first stage of phthisis, the proposition that phthisis most often existed without tuberculosis, but that tuberculosis was liable to supervene in established phthisis, seemed false and confusing. The confusion was one only of names. Niemeyer meant by phthisis the presence of inflammatory consolidations in the lungs, and that these, when caseated, were liable to develop in adjacent or distant parts a vitiated new formation—gray tubercle. What is gray tubercle? As late as 1871 it was said by Walshe,[3] "Attempt to fix the microscopic nature of tubercle is impossible." On the contrary, in 1873 we are told[4] "the knowledge of tubercle is now tolerably complete, and that there are no elements in the induration of phthisis not to be found in the healthy lung."[5]

Robin, Empis, Rokitansky, Rindfleisch, and others, regard gray tubercle as a hypergenesis or inflammatory overgrowth of normal pulmonary structures, especially the connective tissue, or as granular aggregations of exudation substance. Bouchut[6] describes tubercles as "fibro-plastic granulations;" and Empis[7] proposes for miliary tubercle the name "granulie," and for tuberculosis the "granular disease."

[1] Paper before the French Academy, about 1860.

[2] Niemeyer, American edition, "Text-book of Practical Medicine," vol. i., chap. xiii.

[3] Walshe, seventh London edition, 1871.

[4] T. Henry Greene, "Introduction to Pathology and Morbid Anatomy," London, 1873.

[5] Burdon-Sanderson, Reports of Pathological Society of London, *Lancet* of August, 1873.

[6] "Traité des Maladies des Nouveau-nés," etc., Paris, 1862.

[7] "De la Granulie, ou Maladie Granuleuse," Paris, 1865.

Experiments for the development of tubercle by artificial infection have led to the assertion of a still more definite structure. Villemin, in 1865, Simon, Fox, Sanderson, Cohnheim, Klebs, and many others,[1] in 1867 and 1868, successfully developed tubercle in animals by the introduction to the circulation of various foreign septic matters, as well as soluble inflammatory products. The deduction from these experiments was, that purulent or caseous matter in the lungs or in any part of the body might, if absorbed, infect or contaminate the blood, and develop tuberculosis, the same as when inoculated. Infection explains why phthisis is frequently developed by the blood-disorganizing fevers, by the protracted suppuration of wounds and abscesses, by caries and necrosis.

The process of infection is considered to have a confirmation in the advanced knowledge of the histology of the lymphatic system.

Baillie,[2] in 1794, mentioned the resemblance of gray tubercle to gland-tissue. Williams,[3] in 1828, believed its uniform shape and size were due to some peculiar anatomical structure. Virchow[4] so far noticed the resemblance that he ranked gray tubercle, a new formation, among the "lymphomata." Hérard and Cornil[5] consider it a gland-like neoplasm. The existence of capillary lymphoid tissue in the healthy lungs is an established fact of histology. It is variously designated adenoid tissue, the lymphoid capillaries, the perivascular lymph-canals, and is fully described by Recklinghausen in Stricker's "Histology," of which there is an American translation.[6] That gray tubercle is, in many cases, an *inflammatory development upon this lymphoid tissue*, was first asserted by Buhl[7] as early as 1857. His views are sus-

[1] All enumerated. Wagner's "Allgemeinen Pathologie," pp. 478–485.

[2] *Op. cit*, pp. 36, 37.

[3] Stated by Dr. Williams, *Lancet*, August and September, 1873.

[4] "Cellular Pathology," 1857.

[5] Hérard and Cornil, "La Phthisie Pulmonaire," 1867.

[6] "Manual of Histology," by Prof. Stricker, p. 215, etc.

[7] "Zeitschrift f. Rat. Med.," 1857, vol. iv., p. 49.

tained by Billroth, Frey, Klebs, Fox, Sanderson, and others, who consider miliary tubercle a reticulum or net-work of lymphoid tissue, whose apparent nuclei are, in fact, exudation or white blood-corpuscles, or nuclei developed by their proliferation. Histology recognizes also the termination of the perivascular lymphoid canals in certain lymphoid spaces of the walls of the vesicular alveoli. Schüppel[1] believes tubercle may be an inflammatory infiltration of these spaces, developing "giant myeloid cells"—large, multinuclear masses of protoplasm, of irregular contour, and having processes directly connecting with the lymphoid reticulum of the blood-vessels. Two facts, unexplained by any other theory of tubercle, would seem elucidated by the office of the adenoid tissue: 1. That miliary tuberculosis, without caseous masses, is so frequent in infants and young children; 2. That miliary tuberculosis, whether in children or in adults, so often leads to death, without softening or any destruction of lung-tissue. The glandular system of children is susceptible to very slight irritations and to impoverished states of the blood. The preservation of form and consistency is the result of definite structure.

This view of the pathology of tubercle does not ignore the greater liability to phthisis of those who have an unfavorable family record, an inherited defect of constitution. But it denies an exclusive or specific dyscrasia, and explains many cases of tuberculosis, in those who had robust ancestors and previous personal health and vigor, from extreme violations of hygiene, from depressing disease, surgical causes, etc.

These views are held not only by their German originators (too often rewarded for their scientific research by the stigma of "extremists"), but by leading authorities in England, by Fox, Sanderson, Powell, Greene, Williams, Pollock, and Bastian.

Etiology.—We recognize *predisposing* and *exciting causes* of phthisis.

Many have regarded specific *tubercular diathesis*, or *hereditary taint*, as the only *predisposing* cause. Others have

[1] "Lymphdrüsen-Tuberculose," Dr. Oscar Schüppel, Tubingen, 1871.

taken a middle ground; they recognize the tubercular diathesis in many cases but claim a relationship of struma or scrofula, as a predisposing cause of many more. Modern etiology asserts that scrofula and tuberculosis are one in nature, if not in degree, and denies a peculiarly tubercular diathesis. Even Louis recognized the lymphatic temperament as predisposing to phthisis, but in no direct and true sense, for struma, scrofula, and impoverished states of the blood, were considered impotent to produce phthisis, unless a true tubercular diathesis preëxisted. Walshe[1] condemns the idea that "a 'below par' state of health favors the essential vice of nutrition developing tubercle." The old idea is thus expressed by a modern advocate:[2] "It may be enunciated as a fact that every child of a consumptive parent is, from its birth to its death, threatened with the same complaint."

When a disease is unusually prevalent, it is natural to suspect communicability, rather than varied and common causes. Thus phthisis was once regarded as contagious. Morgagni, in 1760, feared to study its lesions; Portal would not attend autopsies of the tubercular. Others considered it contagious only among relations or persons closely associated; and at the present day, in some communities, the bedding of the deceased consumptive is burned. A far better explanation of the prevalence of phthisis was found in hereditary taint.

But, the recognition of the inflammatory forms of phthisis at once set apart a large class of cases as of non-specific origin. Aitken says, "In each generation much phthisis is non-hereditary." And, finally, of the truly *specific nature of any class* doubt exists, strengthened by the writings of Simon[3] and others on the relationship of scrofula. It is a significant fact that the nomenclature of diseases adopted by the Royal College of Surgeons[4] includes in a common group, termed

[1] "Diseases of Respiratory Organs," fourth London edition, p. 464.

[2] Inman, "Restoration of Health," London, 1870.

[3] "General Pathology," first edition, Lecture ix.

[4] "Nomenclature of Diseases," constitutional class.

the "tubercular order," scrofula and phthisis. The two dyscrasiæ are thus ranked as members of one family.

All efforts to prove, by statistics, the existence, in a majority of the consumptives, of an unfavorable tubercular family record, have failed.

Dr. Cotton[1] analyzed 1,000 cases at the Brompton Hospital, and could prove hereditary taint in but 367. Scott Allison's observations, at the same institution, were equally negative.[2] Walshe, by careful inquiry among the phthisical, concludes that not over 26 per cent. have had parents affected with phthisis. M. Pidoux[3] says "not over 25 per cent. of those born of consumptive parents themselves become so."

But let us remember that the occurrence of many cases of pulmonary phthisis, in the successive generations of a family, is not a proof of a specific cause. Possibly all were of the simple inflammatory form, with no connecting link. It is easy to understand why phthisis is so universal; it is liable to result whenever the health is depressed, the nutrition is impaired, or the blood is invaded by septic matter. Waller,[4] Cohnheim,[5] and others, have shown that all structures are developments of preëxisting elements of the blood. All new cells, whether they grow to normal tissues, or terminate in morphological products, are at first leucocytes. There is then no "tubercle corpuscle," no "typhoid cell," no "pneumonic or exudation corpuscle"—each is the same white blood-corpuscle whose subsequent changes are the result of healthy growth or pathological aberration.

So, of tuberculosis, Simon[6] says, "The child inherits an imperfect pattern of development, ... a disposition to form blood in a manner which shall give tubercle as a collat-

[1] Richard Payne Cotton, 1858.

[2] Paper in 1848, "Observations at Brompton," quoted by Aitken.

[3] Quoted by Dr. Durant, Paper on "Curability of Phthisis," New York Medical Society Transactions, 1871.

[4] *Philosophical Magazine*, 1846.

[5] Virchow's "Archives," vol. xl., 1867, p. 1.

[6] "General Pathology," American edition, 1852, p. 128.

eral phenomenon." He regards the scrofulous deposit in the cervical and other lymphatic glands as identical with the material of pulmonary and other visceral tubercle; scrofula, though far more prevalent, he ranked as of lower degree, and as a soil in which tubercle may grow. The latest pathology and the most recent authorities approve and emphasize this view. In the words of Billroth,[1] "only the tendency to chronic inflammation ending in suppuration and caseation is hereditary; the scrofulous diathesis, not the tubercular, is hereditary." In the words of Paget,[2] "the relationship between the two is that scrofulous constitution implies a peculiar liability to the tuberculous diseases." Again: "We believe," says Wilks,[3] "that tubercle is the secondary form of the disease of which scrofula is the primary form." But the denial of a *specific diathesis* is not the denial of hereditary influences, nor of acquired dyscrasia, as predisposing to phthisis. Thus of the lymphatic temperament, and lymphoid origin, Bastian[4] says, "A man may inherit from his ancestors lungs which contain, within themselves, the elements of weakness—organs, the tissues of which are so constituted, that the very slightest developing causes suffice to initiate a set of changes which terminate in one or other of the forms of pulmonary phthisis." Of scrofula says Wilson Fox: "In a family of strumous children, one may have enlarged glands due to carious teeth, another has tubercular meningitis, another later in life has phthisis, following pneumonia or catarrh; the cases and pathological conditions are alike except in result."

It is thus conceded that, in mal-nutrition of the blood, histological elements are impressed with an imperfect type of organization, and result in the low products of struma and tubercle. Therefore, we must look for the *predisposing* causes of pulmonary phthisis among the known existing

[1] Billroth's "Surgical Pathology," American translation, p. 383.

[2] "Surgical Pathology," pp. 715, 716.

[3] "Pathological Anatomy," p. 188, Wilks & Moxon, London, 1875.

[4] "Discussion before the Pathological Society of London," *Lancet*, August and September, 1873.

causes of mal-nutrition—bad air and food, bad assimilation and excretion, and all violations of sanitary law.

The lower animals, when unnaturally confined, develop phthisis. M. Hazard[1] found that cows in the stables of Paris often were consumptives. Dr. Crisp[2] states that phthisis was once frequent among the cows of London, but has been greatly lessened by sanitary changes. Simon found the animals in the Zoölogical Gardens frequently dying of phthisis, including many species free from this disease in their natural state.

Dr. Crisp, in repeated examinations, found no tubercle in the lungs of animals dying in their habitat.

Dr. Henry MacCormac,[3] of Belfast, is the well-known advocate that "rebreathed air" is the chief cause of phthisis. Dr. Pollock[4] considers "deficient ventilation and crowded apartments eminently productive of tubercular disease." Dr. Christison's[5] statistics show the depressing influence of city life, especially among the lower classes. In Edinburgh, as compared with the country, the mortality from all causes was as four to three, the deaths from phthisis were nearly as two to one. The annual mortality from phthisis in a population of 100,000 was, in Glasgow, 385; in Edinburgh, 283; in the Highlands, 179; in the Lowlands—agricultural districts—104 to 138. Waters,[6] of Liverpool, and many others, give the same testimony. Vitiated air is not the only cause of the greater prevalence of phthisis in cities.

The relative inactivity of city-life, and the constraint of many of its pursuits and trades, result in deficient chest-expansion and diminished volume of the lungs. The protective influence of active exercise is marked. M. Lombard found phthisis occurring in 141 of those whose life was sedentary;

[1] "Annales d'Hygiène Publique."

[2] Pathological Society, London, 1873; *Lancet*, September.

[3] "Consumption as engendered by Rebreathed Air," second edition, London, 1865.

[4] "Prognosis in Consumption," London, 1865.

[5] "Address before the Social Science Association," Edinburgh, 1863.

[6] "Diseases of Respiratory Organs."

in 89 of those more actively engaged. The chest-expansion may be regarded a determining cause in resisting and keeping inoperative many combined unhygienic influences which, with neglect, would lead to pulmonary disease. Phthisis is well known to attack those parts of the lung which in health, for anatomical and physiological reasons, expand least. When the constitution is enfeebled or subjected to deleterious surroundings, such parts are least able to repel disease, by reason of slow circulation and often passive congestion. The apex of the lung is the part which is least expanded, and most often the seat of consolidation. In 4,530 cases examined by Pollock, deposit began at the apex in all but 64.

Certain trades cramp the chest; others poison the atmosphere and develop dyscrasia; many more, by volatile emanations and irritating particles, produce catarrhal and interstitial inflammations.

M. Lombard, Marc d'Espré, and Benoiston de Château-neuf, have variously stated the mortality from phthisis in the poor as twice or three times that among the rich. Undoubtedly the knowledge and observation of hygienic principles among the rich add to their health and longevity. The poor suffer not only from their poverty, but also from their improvidence, their ignorance, and neglect of the simplest sanitary laws. That extreme privation and constant errors in quantity and quality of food conduce to develop or intensify dyscrasia, will hardly be questioned. Bennett[1] and Dobell[2] go further—the former assigning acidity of the alimentary tract, the latter deficient pancreatic secretion, as a starting-point of phthisis. They agree in the belief that failure of the intestinal fluids to digest fats causes a deficit in the chyle, incomplete elements of the blood, and results in tissues capable only of retrogression, since they are wanting in the normal proportion of fatty to albuminoid substance. Their views, however, are not established.

We have reviewed the chief predisposing causes of phthisis.

[1] "Pulmonary Phthisis," 1853.

[2] "Tuberculosis," second edition, London, 1866.

It remains to consider the *exciting causes*—the inflammatory thoracic diseases.

Louis affirmed that pneumonia, pleurisy, and bronchitis, had no influence in developing tubercular phthisis.[1] But few persons will take so extreme a ground. Watson, in quoting Louis's assertion, as above, terms it a most dangerous doctrine, and says, "Dormant predisposition is often awakened into actual disease, and latent tubercles are often accelerated by inflammation of pulmonary tissue."

As regards pneumonia, it is the general experience that uncomplicated cases, in healthy persons, completely resolve and leave no disease—a remarkable fact, when we remember the frequency of pneumonia, and that a hepatized lung contains about two pounds of inflammatory exudation.[2] But it is equally true that in periods of influenza, when broncho-pneumonia develops, and in seasons when pulmonary inflammation assumes a typhoid type, we have not only a greater fatality, but sequelæ which often advance to phthisis. When pneumonia is interstitial, and its resolution is incomplete, we have material for pathological action.

The special influence of bronchitis in the production of phthisis has been and is denied by many. Louis said, "The female sex, most disposed to phthisis, suffers least from bronchitis."

Waters thinks cold and damp are minor causes, since phthisis preferably attacks the in-door worker.

The tables of Buchanan, of Forry, and others, are intended to show that phthisis is most fatal inland, and less prevalent on coasts, where bronchitis is most common. Such statements and statistics do not disprove the causative influence of bronchitis. Thoracic inflammation, in any form, is harmless, if the vitality of the system insures a speedy and perfect resolution; danger arises only from permanent infiltration of the tissues, or accumulation of exuded elements in the smaller tubes and the vesicles. Bronchitis occurs at one or many

[1] Watson, p. 723, American edition.

[2] Investigations of Dr. Stiles's paper, *New York Medical Record.*

periods of nearly every person's life. It is the power to surmount it and avoid its frequent recurrence, which determines its insignificance or its gravity. The male has better health, a more robust physique; the out-door worker has active circulation and expanding lungs; the dweller on coasts breathes a more stimulating air. In such subjects, bronchitis more certainly ends in recovery, speedy and complete, less often lapsing into subacute and chronic forms.

But argument is unnecessary, since pathological demonstrations are frequent of broncho-pneumonia, the impaction of catarrhal products, the occlusion of smaller bronchi and degeneration of lobules, and general infiltration of the fibrous framework of the lungs—following the bronchitis of the young, the old, the feeble, and the subjects of asthenic disease and slow convalescence.

Irritation of the throat, by continued cough, causes bronchial hyperæmia and bronchial catarrh. Hence throat-diseases may contribute toward the ultimate condition of phthisis.

Next to the lungs, tubercle, Billroth tells us, is most frequently located in the larynx.

The *post-mortem* frequency of pleuritic adhesions has always been noted. In Matthew Baillie's "Morbid Anatomy," 1794, they are termed "the most common morbid appearance in dead bodies." This is the personal experience of every physician.

Occlusion of a pleural cavity is exceptional, partial occlusion is not infrequent; adhesion bands and fibres, intact or ruptured and atrophied, plastic patches and thickened pleura, are common.

When not absorbed or broken down by the respiratory movements, membranes or bands often organize; they may become vascular; and arteries of size, as traced by Van der Kolk and Guillot, may pass through the connecting adhesions, from the lung to the thoracic wall. The clinical frequency of local and slight pleurisy is attested by Walshe; "evanescent pleurisy," he says, "is of the most frequent occurrence."

Dr. J. R. Leaming, of this city, in his several published monographs, has put on record many cases of local dry pleurisy, and detailed the physical signs by which they are to be positively recognized.

Laennec, in 1819, asks, " Can tuberculosis be a termination of pleurisy ? " and adds, " The proposition is absurd, for it is preposterous to suppose that inflammation of one organ should terminate in another."

Yet many clinical records testify to the occurrence of local pleuritic pain, and confirmatory physical signs of local pleurisy, preceding any symptoms or physical signs of pulmonary deposit, and when chest-expansion and perfect vesicular element of respiratory sound proved the lung to be intact.

Conceding, as is generally held, how often pulmonary tubercle is primary and leads to a local and secondary pleurisy, does not the frequency of local and evanescent pleurisy, and the common occurrence of adhesions in the cadaver, justify the opinion that in many cases they may be the antecedents and cause of phthisis ? Louis[1] found, in 112 cases, only one in which the lungs were free throughout their whole extent. Broussais[2] claimed to have seen pleurisy the precursor of tubercle in many cases.

Swett[3] thinks that phthisis is more often due to pleurisy than to pneumonia or bronchitis.

Dr. Leaming[4] is well and widely known as an advocate of the frequent pleural origin of phthisis.

Rindfleisch,[5] under the head of " Pyrogenous Pneumonia," concedes that "pleurisy may precede and be the cause of pulmonary infiltrations ; " the outer row of infundibuli are often filled with exudation.

Histology and pathology, as well as observation of symp-

[1] *Op. cit.*

[2] " Chronic Phlegmasiæ," vol. i.

[3] " Diseases of the Chest," 1856.

[4] " Plastic Exudation within the Pleura."—Dr. Brown-Séquard's " Archives," March, 1873.

[5] " Pathological Histology," p. 417, American edition.

toms and physical signs, will support this view. There is an intimate vascular connection of pleura and lung, and through established adhesions often there is active collateral circulation; thickening and adhesions of the pulmonary pleura result in irritation and hyperæmia of the lung. Persistent hyperæmia results in hyperplasia or in infiltration, which may degenerate and become crude tubercle. The influence of pleural adhesions, by causing immobility of the lung, in favoring tubercular processes, is stated in the recent revised edition of Jones and Sieveking's,[1] though regarded essentially secondary.

The usual location of phthisis, at the apex, accords with the view that pleural adhesion may sometimes be its cause. By inflammation, the pleura is engorged and tumefied, is deprived of its epithelium, and presents a villous, granular surface, whose opposed walls are liable to unite.

This may occur at once when exudation is limited to subserous infiltration. But, as a rule, there is an escape of serum or sero-plastic matter into the pleural cavity. The liquid exudation gravitates and keeps the middle and lower surfaces separated, but the upper are collapsed, in apposition, and unite their granular, villous processes. This process of pleuritic adhesion Rindfleisch compares to the union of the opposed walls of a granulating wound. Such adhesions, when formed, may be speedily ruptured and absorbed; but, when permanently located and organized, they are a source of irritation and restraint to the lung through the person's future life. Will not an irritation of lung-surface, renewed eighteen to twenty times per minute, result in pulmonary hyperæmia and progressive consolidation? Will not the extent of peripheral nerve-irritation in pleurisy tend to depress the general health just as superficial burns and scalds are a source of shock, and extensive irritable ulcers lower the innervation of the whole body?

The reduction of the lung to a cirrhotic, carnefied state

[1] "Pathological Anatomy," p. 511, Jones and Sievking, by Payne, London, 1875.

by contracting thickened pleura, the presence of caseous nodules in masses of pleuritic exudation, are less frequent but recognized dangers of pleurisy.

As inflammation of the cerebral meninges, the pericardium and peritonæum are sources of injury to the organs they invest, so the pleura when inflamed may result in consolidation of the lung.

Climate, whose extremes and fluctuations are the immediate causes of these thoracic inflammations, has a direct relation to the prevalence of phthisis. It is a disease of temperate regions, but little known in the colder and more northern ones, and comparatively infrequent in tropics. It prevails where the temperature is most changeable. Removal from warm to colder climates, as from the Indies to Europe, or from the Southern to the Northern States of the Union, often develops it.

The United States census for 1870 shows the geographical correspondence of the distribution of phthisis with the existence of cold and changeable climate. In the different parts of our country, the mortality from phthisis varies between one-fortieth and one-fifth of the deaths from all causes—the greater mortality coinciding with the colder localities and those most subjected to climatic change. This report affords a controversion of the statistics of Forry and others, as to bronchitis. We find in California a seaboard district having a maximum mortality from phthisis, and an adjacent interior where the disease attains a minimum.

"Phthisis ab hæmoptoë" was an expression frequently and fallaciously employed, previous to the scientific physical exploration of the chest; and yet the term is applicable to many cases of consumption, in which ill-health and pulmonary deposit, as indicated by physical signs, were preceded and induced by an attack of hæmoptysis. Bronchial arteries, as demonstrated by Cammann,[1] by Waters,[2] by Schultze,[3] and

[1] *New York Journal of Medicine.*

[2] Prize essay on "Anatomy of the Lungs."

[3] Article on "The Lungs," Stricker's "Histology."

others, return only a part of their contained blood to the bronchial veins; the remainder passes—not by capillary anastomosis—but by direct intercellular arteries to the branches of the pulmonary vein, and to the left side of the heart.

Hæmoptysis—usually bronchial blood—may occur in perfectly healthy persons, whose lungs are intact, from excessive effort and extreme cardiac excitement, but more often, the lungs being healthy, as a result of mitral insufficiency. Again, any pulmonary deposit, though of a harmless nature in itself, may obstruct these intercommunicating vessels in adjacent tissues, causing a local engorgement of a bronchial twig, and hæmoptysis. Hæmoptysis, from conditions of local congestion, is often a relief or positive benefit, provided clotted blood be not retained in the bronchi.[1] The lodgement of blood in the bronchi, from any cause, is disastrous to the integrity of the lung, exciting local broncho-pneumonia, and exerting a septic influence on the general health; rapid infiltration and caseation are its frequent and fatal sequelæ.

The danger is well illustrated by the fact that broncho-pneumonia often results from the accidental entrance of blood into the bronchi, when tracheotomy is accompanied by hæmorrhage, or any operation upon the throat is performed under anæsthesia. Profuse hæmoptysis demands arrest, but a slight attack is less to be feared than the dangers incident to hastily checking it—the development of a new focus of caseous tubercle.

Having thus reviewed the pathology and etiology of pulmonary phthisis, as a basis of prevention and early arrest, we may be guided by two conclusions:

1. Dyscrasia, or predisposition, is largely the cumulative result of depressing influences, which sanitary control and personal regimen may diminish.

2. Inflammatory attacks are the chief exciting causes of pulmonary phthisis, whether in the strumous or in the previously healthy; causes which may be largely averted by selection of climate, and avoiding the exposures which lead to

[1] Niemeyer, vol. i., chap. xiii.; Leaming, "Hæmoptysis."

them, or rendered trivial by constant care and supporting treatment.

Prevention.—The prevention of phthisis is a question—

1. Of public hygiene.

2. Professional supervision of persons and families in whose cases predisposition—inherited or acquired—exists.

1. All public sanitary measures, as the prevention of overcrowding, the vacation of unhealthy abodes, the enforcement of tenement-ventilation, the correction of defective sewerage and drainage, the public inspection of food, and the removal of all known causes of blood-poisoning, will have an ultimate influence in lessening the extent of the dyscrasia, in correcting the lymphatic temperament, struma, or scrofula, and an immediate influence in preventing the asthenic diseases and low types of thoracic inflammations, which may develop, during convalescence, the degenerative processes of phthisis. The sanitary reforms in London have increased the average longevity from thirty-five to forty-one years.[1] The favorable effects of hygienic conditions upon the very worst class of lives may be seen by a study of the various public institutions for poor children. Asylums for foundlings, for orphans and half-orphans, contain the offspring of parents who have died of phthisis and of other diseases engendered by privation and vice: reclaimed from homes of poverty and neglect, they have inherited all the conditions of struma. Yet in one institution there will be a constant recurrence of scrofulous disorders and frequent deaths from miliary tubercle of the brain, the lungs, or bowels; while another asylum may present a striking contrast of general good health, and an exceptional occurrence of tuberculosis.

In a miniature community, then—through the agency of diet, clothing, exercise, and proper housing — meningitis, phthisis, scrofulous glands, eczema, and purulent ophthalmia, have been measurably controlled.

In the community at large, public hygiene will likewise lower the scrofulous or tubercular diseases, exerting a marked

[1] Since 1600. Dr. Stephen Smith, "Public Health Reports," 1875.

effect on the susceptible organism of growing children, and a corresponding influence upon adults. Of our adult foreign population, how many there are who can date their pulmonary trouble from the noxious exposures of the ship, and landing at a season and in a climate whose severities of temperature they were not prepared to withstand! Native and foreign alike suffer from the defects of the tenement-house, the factory, and the shop. The places where the poor dwell and work must be purified by light and air. There are questions of public hygiene equally vital to the rich; such are the proper location and building of the home, the town, the city, proper sewerage, the purity of the water-supply, and the hygiene of the public schools.

2. The guidance of the individual for the prevention of phthisis may begin at birth—the nursing and airing of the infant, the diet of the bottle-fed and weaned, the management of first dentition, the treatment of summer diarrhœa, and the eruptive fevers, so as to leave no catarrhal sequelæ or reduction of vigor.

But, in those in any way predisposed, it is at adolescence, in approaching maturity, that advice and direction should avail most—to the too-assiduous student, the youth ambitious for business advancement, the young woman entering society. The family history may well be reviewed, the personal temperament and physique considered, the safety of the proposed vocation estimated. I believe it a duty to dissipate the discouragement which often exists through a belief in the power of a specific, hereditary tubercular taint. But it is no less a duty to clearly depict the dangers which arise from incorrect habits of living, neglected action of the lungs, and colds; and to cast a horoscope of the future which is in store, unless the preservation of health is a constant study. I can recall many instances of most decisive results from such advice. Directions as to personal regimen have been followed in detail. The diet is to be judiciously selected, easy of assimilation, and in quantity avoiding the excess which taxes the digestion and loads the alimentary canal, since chronic dyspepsia and con-

stipation impoverish the blood. The value of a healthful state of the skin cannot be over-estimated—not alone its cleanliness and functional activity in elimination, but rather its normal temperature and perfect circulation, as protections from visceral congestion. The statistics of Edward Smith[1] on the protective influence of proper clothing accord with my experience in several recent cases, in which covering the general surface in flannel accomplished a tonic effect which other means had failed to obtain. The general clothing should be suited to the season, with provision for adapting it to variations of temperature from day to day.

No physician, who has not made it a business to inquire closely, can be aware of the irregularities, the errors, and neglect of the proper protection of the body which exist in every class of society. Injudicious and too frequent bathing is dangerously sedative to those who are strumous or in delicate health, and lack the vitality for vigorous reaction. The baths of such persons should be stimulating, by the addition of sea-salt, and followed by thorough friction.

The full development of the chest, and the increase of its expansive measurement to a proper standard, must be accomplished either by systematized methods of chest-exercise and inhalation, or by a business pursuit which secures the same result. The flat surface must become rotund, and feeble respiratory sound be replaced by the rhythm and volume and the vesicular element which characterize its healthy state.

Exercise and out-door life must be secured in choosing a vocation—the life of a farmer or horticulturist, the profession of mining or civil-engineer. The gymnasium is less valuable than active work, walking, athletic sports, and riding on horseback. Sydenham is reported to have said, "In the treatment of consumption the best physician is a horse, and the best apothecary is an ass."[2]

It is too often true that, in advanced stages of pulmonary phthisis, the physician can do little to control its progressive

[1] Edward Smith, "Consumption, its Early and Remediable Stages."

[2] Motherby's Dictionary of Medicine, "Consumption."

tendencies. But its early arrest, when presenting its first distinctive symptoms and physical signs, is to be attempted, and in very many cases, through persistent effort, will be accomplished. There are both methods of procedure and medicines which cure, or control the progress of, the incipient stages of phthisis. These stages are varied in nature—in one instance being a purely local lesion of the lung, in others complicated by dyscrasia. Manifestly, the treatment in either state must never be other than supporting. To increase the nutrition of the blood, lessens the danger of new inflammatory deposits, and the liability of those already present to degenerate. The arrest of phthisis, when possible, will be chiefly by three agencies:

1. *Food*—including those articles which are most highly nutritive, and those remedies which conduce to its thorough assimilation.
2. *Chest-expansion* as a means of fortifying the lung against renewed inflammatory attacks.
3. *Climate.*

The diet must possess the qualities already enumerated in considering prevention, and digestion will require no less the stimulus of air and exercise. These may suffice. Autopsies of aged persons show in many cases the proofs of spontaneous cures. But cod-liver oil, as an agent of nutrition, is recognized as most curative of all remedies for phthisis. It is the richest of the hydro-carbons. In dynamic, or force-producing power, as estimated by Pavy,[1] it takes precedence of all other aliments. Cream will not nourish so rapidly. Pancreatic emulsion is inferior in benefit. It has no substitute. Of its mode of action there are many theories. Certainly it increases the general nutrition of the blood, for its use extends to other diseases than phthisis.

The residents of northern coasts who subsist largely on fish and fats are reputed free from tuberculosis. Bennett claims a perceptible decrease in the mortality from phthisis in Scotland since the use of oil in its treatment.

[1] Pavy on "Food and Dietetics," London, 1874.

Cod-liver oil may often have its efficacy increased by other remedies. Phosphates may be incorporated with it, as supplying an element of normal nutrition. Pancreatine will insure its digestion and absorption. Alcohol will aid its assimilation in greater quantity.

Campbell,[1] Thompson,[2] and Walshe, state that ozone or oxygen, incorporated with oil, lowers the frequent pulse fifteen beats, and aids nutrition. Iodine, a well-known alterative in scrofula, may be employed when diathesis exists. Tonics of every kind coöperate in the nutritive treatment.

Chest-expansion is the great means of securing the functional activity of the uninvaded portions of the lung, and insuring the oxygenation of the blood.

Climatic treatment has for its chief purpose a studied avoidance of colds, and their speedy resolution when acquired.

Changes of residence, either temporary or permanent, with reference to the benefits of climate, are permitted by the circumstances of but few consumptives. We are forced, therefore, in the majority of cases, to combat at home the inflammatory disorders which our severe winters excite. Ammonia and quinine are preëminent remedies in accomplishing this purpose. Ammonia is a diffusive stimulant, is almost exclusively eliminated by the lungs, and favors the escape of hyperæmic products in the form of mucus.[3]

Quinine limits exudation, reduces temperature and danger of caseation, and hastens absorption of inflammatory products.

[1] Walshe, "Diseases of the Lungs."

[2] "Braithwaite," part xi., p. 294.

[3] Woods's "Therapeutics," Philadelphia, 1875, *et al.*

CASE OF MULTIPLE ABSCESS OF THE LIVER.

By SAMUEL W. DANA, M. D.,
LATE SURGEON TO THE NEW YORK DISPENSARY.

Read July 1, 1875.

The case of "Multiple Abscess of the Liver," which I have the honor briefly to report this evening, was seen during some part of its course by several Fellows of this Academy, and is believed to possess some points of rarity and interest.

The patient—a widow lady, fifty-two years of age, engaged during the previous ten years in active business pursuits, and during this period enjoying good general health—was attacked on the 3d of June, 1873, after unusual indulgence in eating, with severe epigastric pain and persistent vomiting. These symptoms, partially relieved during abstinence, were excited anew by every attempt to introduce even the blandest form of nourishment into the stomach.

After a few days, pain in the right hypochondrium extending around to the back began to be complained of, and a slight bilious tinge appeared upon the skin. The bowels were constipated unless acted on by medicine or injections.

On June 22d the patient was seen by my friend Dr. Purple. She had then been eighteen days sick. Her condition was one of great exhaustion. There was also decided emaciation. From this point, however, she began slowly to improve, and by the middle of July was sufficiently recovered to leave for the country. Her convalescence was apparently perfect. Subsequently, for more than a year, she was in the enjoyment of excellent health.

On the 14th of September last (1874), nine months and

three days before her death, the patient complained of pain in the head and back, with a feeling of general *malaise;* said she had spent some time in the park the previous evening, and thought she had taken cold. The following day she felt as well as usual. On the 16th she suffered an attack similar to that of the 14th. In reply to inquiries, she said that she had never suffered from fever and ague; but in July of the previous year she had spent two weeks in Greenwich, Conn., a malarial district.

On the 18th she had a chill, followed by fever and sweating. Quinine was then given, and the chills interrupted for the time.

On the 17th of October, after partaking of a hearty dinner, the patient was attacked with severe epigastric pain and vomiting. During the next ten days the attack much resembled that of June of the previous year, being characterized by occasional severe epigastric pain, vomiting, inability to take food, rapid emaciation, and loss of strength. There were also some pain and tenderness in the right hypochondrium, and paroxysms of chills and fever, though nearly controlled by quinine, showed a tendency to recur every second day.

Dr. Purple saw the patient on the 28th of October, and occasionally afterward during the next three weeks. At the above date a slight yellow tint had appeared on the skin, which in the following days rapidly deepened to that of intense jaundice. Bile disappeared from the fæces, while the secretion of the kidneys, which was scanty, resembled, both in color and consistence, bile rather than normal urine.

With the development of jaundice the pain and tenderness in the right hypochondrium increased, and a chill came every forenoon with an exacerbation of fever in the evening; the temperature, as indicated by the thermometer in the axilla, ranging from $101\frac{1}{2}°$ in the morning to $103\frac{1}{2}°$ in the evening. At this period slight fullness in the region of the right hypochondrium was noticed. The free margin of the liver extended about two inches below the cartilages of the ribs, and yielded on palpation a feeling of increased resistance.

At this time the case was regarded as one of malarial fever which had passed from the intermittent to the remittent type. The jaundice remained quite intense for about a week, then gradually declined. The pain and tenderness in the region of the liver diminished, and the fever abated; the temperature during the fourth week varied from 100° to 101° at the evening exacerbation. It was during the decline of the jaundice that a new symptom appeared which came into somewhat prominent notice during the subsequent course of this case, namely chills, often slight, sometimes quite severe, occurring with irregular intervals, usually one or two, sometimes three or four, in the course of twenty-four hours.

During the third week of November—the fifth week of the attack—the condition of the patient was far from promising. Chills were of frequent occurrence. Coldness of the extremities made necessary the application of artificial heat during a considerable part of each twenty-four hours, while a slight evening exacerbation of fever was followed by copious sweating during the earlier part of the night. There were great emaciation and weakness, and marked sallowness of complexion. The lips showed great pallor. The pulse was quite feeble and rapid; the tongue pale and dry; a frequent, short, dry cough began to harass the patient, her feet and legs became œdematous, while the facial expression assumed such a character as to add to the solicitude which the symptoms just mentioned had aroused. This condition of extreme prostration seemed partly due to the patient's inability to take and appropriate nourishment. The stomach had been quite irritable from the first; milk, eggs, and alcoholic stimulants, it rejected utterly. Beef-tea was the only nourishment taken during the first four weeks, and that in amount quite inadequate. But even that the stomach now rejected unless the solid particles constituting its sediment were first strained out, leaving it nearly worthless. Various extracts of beef made into tea were next tried, and though tolerated by the stomach were found in point of nutrition quite insufficient. Beef-tea prepared after Liebig's method was tried with better success.

The stomach bore it well and in sufficient quantity. Its employment at this time seems to have been the means of rescuing the patient. The improvement, however, was quite slow. Frequent chills still harassed the patient, and seemed to neutralize every tendency toward convalescence. By the middle of December, however, substantial improvement had been made. The chills had diminished in force and frequency. The patient had gained in strength. During the last two weeks of December the chills entirely disappeared, and the patient was able gradually to resume her ordinary diet of solid food. From December 22d to January 18th the patient had no marked chill, though she noticed an occasional coldness of the extremities, which caused her to keep near the fire for an hour or two at a time. During this period, however, her convalescence was quite rapid. She was free from pain, ate heartily, and slept well, gained in flesh and strength, resumed her usual avocations, and received the congratulations of her friends on her supposed recovery.

On January 18th a chill occurred. Quinine, of which five grains a day had been given since January 1st, was now increased to ten grains, and, although a tendency to the recurrence of chills occasionally manifested itself, yet during the next four weeks, to the middle of February, the patient enjoyed very fair health. On February 16th, after an indiscretion in eating, she had an attack of indigestion, which kept her in bed three days. From this point during the following four months, to the time of her death, with some brief periods of improvement, there was on the whole a gradual and steady decline. About this time the irritative chills again made their appearance, though slight and of infrequent occurrence. Near the 1st of March, in connection with an attack of indigestion, the patient complained of pain in the right hypochondrium, for which a blister was applied. The pain was of short duration, and this was the only occasion on which she had complained of pain since the previous November. During the month of April there was more frequent recurrence of the chills, with progressive loss of strength and digestive power.

She found it necessary again to relinquish solid food and return to beef-tea, which she now took with reluctance, and in quantities insufficient to sustain life for a prolonged period. About the middle of May, increasing weakness compelled her to take to her bed, from which she scarcely rose during the last four weeks of her life. Increased tenderness over the liver was noticed some days prior to the 1st of June. The abdominal muscles also at times were remarkably rigid. In the first week of June a tumor was detected on the upper surface of the liver, just beneath the cartilages of the ribs, which did not, however, yield a sense of fluctuation. With the exception of the local symptoms just referred to, no new symptom appeared during the last few weeks of life. Progressive emaciation and increasing feebleness seemed alone to mark the progress of the case. But little food was taken during the last two weeks; none during the last forty-eight hours. The patient's intellect remained clear until a few hours before her death, which occurred on the morning of the 17th of June, seven months and a half from the occurrence of the jaundice, seven months from the occurrence of the irregular or irritative chills, and four months from the time of their recurrence in February.

The *post-mortem* examination was made fifteen hours after death—present, Drs. Purple, Clark, Prescott, Campbell, and Dana. On turning aside the abdominal coverings, the lower margin of the liver was seen projecting about two inches below the cartilages of the ribs, and normal in appearance. On attempting to bring the entire organ into view, it was found adherent by its superior surface to the abdominal wall in front, the adhesion being about one square inch in extent. While this was being broken up, although the manipulation was quite gentle, pus spurted from a point on the inferior surface. When the entire organ was brought into view, that portion of the superior surface which *in situ* was covered by the ribs, presented a striking appearance, eliciting the remark, from one of the gentlemen present, that it was clearly a case of cancer. Scattered thickly over this portion of the

surface were slight elevations circular in outline, and from one-fourth of an inch to an inch in diameter. Several of these elevations, aggregated more closely together, constituted the tumor that had been diagnosticated before the patient's death. It was of an irregularly oval outline, about two by two and a half inches in diameter, and with an average elevation of six or eight lines above the general surface of the liver. The scalpel quickly revealed the fact that the elevations referred to were abscesses, and further investigation showed that the right lobe of the liver was filled with abscesses thickly disseminated through its entire extent, varying in size from that of a pea to that of a hen's-egg. The pus contained in them was light colored, and rather thin and fetid. The left lobe of the liver and spleen were normal. The right lobe of the liver was about two-thirds of its normal size.

The *post mortem* having definitely shown the case, at least at its conclusion, to have been one of multiple abscess of the liver, dependent on portal infection, or portal phlebitis, three questions of interest present themselves: 1. What was the source of such infection? 2. At what period did it occur? 3. What was the nature of the chills which formed so prominent and so persistent a feature in this case?

Leaving to others the general discussion of these questions, I shall conclude the report of this case by mentioning a few facts observed during the course of the disease, which may tend in some degree to their elucidation. In regard to the first question, that of the source of the portal infection, unfortunately, facts bearing on this point are meagre. The circumstance that the abscesses were limited to the right lobe of the liver, and thickly disseminated through it, would seem to indicate that the infection did not enter the general portal current, but only that portion of it distributed to the right lobe; and that, therefore, its source was in the liver itself, and not in any other of the abdominal viscera. And this view seems in harmony with the history of the case, no symptom at any time having pointed to suppurative or ulcerative action in any other of the abdominal organs, while early in the case the symptoms

suggested the probability of suppurative action within the liver.

2. At what period did portal infection occur? The history of the case seems to leave this point in some obscurity. In November there were symptoms of suppurative action in the liver, pain, tenderness, hardness of the liver, jaundice, frequent chills, elevated temperature, night-sweats, great prostration, etc. Then followed a period of absence of chills, and of partial convalescence; then a period of gradual decline, lasting four months. If portal infection occurred at any period subsequent to November, it took place very quietly without producing marked impression on the general system, or giving rise to any local symptom as far as is known. As bearing on the question of an early origin of the portal infection, there is to be noticed the very slow rate of progress of the case during the last four months, as indicated by the general symptoms. It had in general the aspect of a case of chronic rather than acute disease. During the last three weeks when we knew that abscesses of the liver were present, the rate of decline was as slow as during the previous four months.

3. What was the nature of the chills? Were they malarial, or symptomatic of suppurative action, or in part both?

The chills commenced September 14th, five weeks before there was any suspicion of local disease. They showed a tendency to recur every second day throughout the entire course of the disease, excepting a short period in November, when they came every day. The irregular chills began during the decline of the jaundice in the second week of November, and continued with greater or less frequency till about the 1st of January, when they entirely disappeared. They reappeared about the middle of February, and continued except when controlled, by treatment, to the close. The effect of quinine in controlling the chills was remarkable. This came especially to notice during the last three months. The patient having come to hate the drug and the disease with an almost equal hatred, on several occasions she suspended the use of the quinine for a few days. The effect in every instance was the

same. The chills, which, while the quinine was being used merely marked their time by a short period of discomfort, would, on the suspension of the medicine, come back with decided severity. A marked instance of this occurred during the patient's illness. On June 1st the patient had concluded to take no more quinine. On the evening of that day—it being the regular period—she had a slight chill. On the evening of June 3d she had a severe chill which lasted till midnight. Before reaction had fairly set in a second chill occurred, more severe than the first. The following morning she was found pulseless, with icy coldness of the extremities, but in fair possession of her mental faculties. Death seemed imminent. She was now willing to resume the quinine. It was administered to her liberally, and reaction gradually set in. After this occurrence the patient lived two weeks, and taking her quinine regularly, there was no recurrence of the chill.

WHAT IS THE BEST TREATMENT IN CASES OF LABOR IN CONTRACTED OR DEFORMED PELVES, RANGING FROM TWO AND A HALF TO FOUR INCHES—FORCEPS OR VERSION?

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Read September 16, 1875.

PART I.

I should be wanting in courtesy and respect if I did not respond to the resolution which was passed at the meeting of the Section on Obstetrics of this Academy, last June, to present in a more extended form to the Academy the observations I made extemporaneously at the meeting of the Section in May last.

I was not aware, until a short time since, that your worthy President desired that I should occupy the first meeting, at the opening of the Academy, after the summer vacation. I realized, from my professional engagements occurring out of the city for two or three weeks, that I should not be able to perfectly fulfill my obligations to the Section, and I am fully aware that these observations are not so complete nor so thorough as I could wish.

The question for consideration and discussion at the May meeting of the Section on Obstetrics was, "What is the Best Treatment in Cases of Labor in Contracted Pelves?"

Instead of this general question, which embraces a considerable field, I propose dividing it into two parts, viz.:

1. What is the best treatment in cases of labor in con-

tracted or deformed pelves, ranging from two and a half or two and three-quarters to four inches?

2. Is craniotomy, cephalotripsy, or cranioclasm, preferable, in a case of labor when the pelvis ranges from two and a half or two and three-quarters to one and a half inch, to the Cæsarean section?

I intend this evening to occupy your attention with the first question, and shall defer the second to another opportunity. Both questions are highly-important, clinical, and practical ones, and I trust the subject is one which will merit a free discussion by the many able and experienced obstetricians present. No point in the wide field of obstetrics has attracted more attention than this subject, and none where such a great difference of opinion has been manifested during the last fifteen or twenty years.

We must concede we are under great obligations to our predecessors for the genius they have displayed and the industry they have shown in investigating the importance of treatment in contracted pelves. So much light has been accumulated at the present day, with the improvements of instruments, the investigation as to the formation of the different kinds of deformities of the pelvis, and the mechanism appertaining to the class of labor incident to their defects, that we can look, I believe, upon the treatment of contracted or deformed pelves from the normal, with more confidence of success, especially for the mother, than apprehension. I shall confine my remarks and observations to a few practical points springing up from my own experience, differing from or affirming the opinions and views of those obstetricians who have given their attention to this subject. To solve the question before us, as to the best treatment, we shall have to consider whether version or forceps be most preferable and judicious, or what are the advantages and success of one over the other?

Two important principles are to be recognized, and these should guide our action:

1. To select such operations as may conduce to the

safety of the mother and child, and avoid craniotomy if possible.

2. To deliver the patient with as much ease and safety, and as early, as correct and judicious treatment to her pelvic structures and her future health may require.

Before expressing my opinion regarding these points, allow me to present the clinical history of two cases of labor, and which are brought forward as typical of those kinds of cases which we occasionally meet with in active practice. One occurred in a uniformly contracted pelvis, and the other in a simple flat, non-rickety pelvis. I shall reserve the history of a case of kyphotic pelvis with a diameter of only one and seven-eighths inch in the inferior strait, and the presentation of a double anchylosed pelvis, the "Robert pelvis;" and the recital of a case of labor in a simple flat pelvis of two inches diameter at the superior strait, to the time when I take up the second part, or contractions from two and a half or two and three-quarters to one and a half inch in diameter.

CASE I. *Simple Flat Pelvis.*—M. C., aged twenty-one years, was taken in labor November, 1868, at 6 A. M., in Bellevue Hospital, with moderately active pains, occurring every fifteen minutes till 9 A. M., when the membranes ruptured and the waters were evacuated, the os uteri opened to the size of half a dollar. By 12 o'clock the os uteri was nearly fully dilated. I visited the patient at 1 P. M., just previous to my lecture at 1½ P. M. A deformity of the pelvis was recognized, having a diameter, as I supposed, of three inches at most; the head presenting, and placed slightly obliquely transverse at the superior strait; pains active. I decided to deliver the patient by version. After being fully anæsthetized by chloroform, and afterward by sulphuric ether, she was brought before the class in the amphitheatre. External manipulation was adopted: after raising the head from the superior strait, and bringing the breech down, the hand was introduced into the vagina, when a knee was recognized and version, by two fingers, accomplished. During the delivery of the body of the fœtus, the back of the child was made to look posteriorly, abdo-

men upward; the occiput directed to the left sacro-iliac space, child's face looking slightly upward, instead, as is usual, downward. Firm pressure was made on the forehead of the child, and the occiput pressed against the latero-posterior part of the pelvis; the posterior part of the child's head occupying the larger or the sacro-iliac space of the pelvis. In this position, by the external pressure on the head, the frontal, as well as the occipital, was made to shelve some little under the parietal bones, thus diminishing the long diameter as much as one-quarter inch; then, with firm, steady and decided pressure the head was forced downward and backward, while the body of the child was brought forward, when the head passed into the cavity of the pelvis, and the child was delivered in the ordinary way after version—face posteriorly, back upward. The tilting obliquely or canting the head is only following the instructions we notice in many cases of labor, as it is not always synclitically placed. The child weighed over nine pounds. The absolute measurement of the child's head was two and a half inches bitemporal after delivery. The indentation was fully half an inch, and the child lived for a few minutes. I am fully aware of the uncertainty of being exact in the measurement of deformed pelves by any instrument or finger. Having made trial of the various pelvimeters, I confess my attachment to the use of the finger after the different positions the patient may be placed in for examination. In my estimate I allowed more for the thickness of the soft structures. Time of delivery, half an hour.

The patient had no unfavorable symptom, and progressed satisfactorily to a perfect recovery.

Case II. *Generally Contracted Pelvis.*—March 4th, 1875, at 9 A. M., I was requested to visit, in consultation with Dr. C. C. Lee, Mrs. S., aged twenty-three, primipara, in labor, and arrived at full term. She was taken in labor on the 3d, and seen by Dr. Lee at 11 P. M. Labor had advanced but little; os uteri not opened. He left her till morning. Summoned at 4 A. M. on the 4th; at 6 A. M. os uteri

dilated so as to admit the finger, and very thin. Pains frequent and of some severity, membranes ruptured, waters evacuated; she remained in this state till I saw her at 9 A. M. Chloroform had been administered on account of the severe pains and the nervousness of the patient. On examination I recognized the head of the child presenting, and the occiput dipping into the superior strait posteriorly. Head well flexed, and covered by the expanded cervix; os uteri opened to the size of a five-cent piece, or seven-eighths of an inch. Full dose of laudanum ordered, forty to fifty drops, to procure some sleep if possible, and to be visited at 1 P. M. Very little sleep obtained. Pains not so frequent nor so severe. As there was no improvement in the labor, patient restless, and the pulse over 100, it was decided to aid the delivery, and for this purpose to apply my long, small, thin-bladed forceps (Fig. 3), instead of resorting to the dilators, or by manipulation on the cervix with the finger in stretching the os uteri. She had now been in labor twenty-two hours, and seven hours from the rupture of the membranes; os uteri the same size as it was at 6 A. M. Chloroform was administered at 2 P. M. The small, thin-bladed forceps applied. The forceps were selected in this case as offering an earlier relief than by version, which latter could not have been accomplished with so much safety nor so soon, I believe, as the cervix was thin and not dilated to the extent of two inches, sufficient to allow the introduction of the hand, unless the two-finger turning, with external manipulation, was carried out. In fifteen to twenty minutes the cervix was sufficiently expanded to withdraw the small-bladed forceps, and apply a stronger and larger instrument, the os having a diameter of two inches, with the occiput partly through the cervix. It was at this stage I suspected a uniformly contracted pelvis. Traction was made slightly downward and backward. During the traction with the large forceps, the cord came down two or three times and was easily replaced. As fully three-quarters of an hour had elapsed since the trial of the forceps, and no advance having been made in the further descent of the head in the cavity, the child being

dead, with the consent of the husband and the concurrence of Drs. Lee and Smith, embryotomy was performed. It was apparent the pelvis was uniformly contracted, with a diameter of about three and three-quarters inches; and as there was no prospect of her being delivered in any other way, the head was perforated with the forceps on, brain evacuated, and traction made, and the child delivered in fifteen minutes. Time of operation one hour and three-quarters ; child weighed eight pounds. There was some slight pelvic irritation afterward for a day or two, and the patient recovered perfectly.

It is my impression that there could have been no better course adopted than this, if we desired the delivery of the patient in the condition the os uteri was in. It illustrates clearly to my mind, and from the experience I have had in the early application of the forceps, that greater benefit will result not only to the mother and the child, as by delay and procrastination, more unfavorable symptoms may arise which would tend to the unfortunate issue of the case. I have never seen any bad results in the application of the forceps in like cases, and some of the patients have been inspected ocularly soon after delivery. On the other hand, two perfect and complete circular amputations of the whole vaginal portion of the uterus have occurred under my supervision, when the delay and neglect to use the forceps was evident. In both these cases, the vaginal portion was examined and measured, in one in some parts one and a half inch, and in the other one inch. In a third instance the measurement was three-quarters of an inch.

In July I saw, with Dr. J. P. White, a case of eclampsia in a case of pregnancy of eight and a half months. The convulsion was very severe, the patient remained comatose for three days, and for a time was partially paralyzed, but recovered perfectly. In this case labor was instituted, and the contraction of the pelvis could not have been more than three and three-quarters inches. The child was obliged to be sacrificed in this case also. The history I have given of the cases of uniformly contracted pelvis, as well as in the one I shall relate in

a kyphotic pelvis, occurred on the same day, and required the same treatment in this early stage of labor. It is an innovation which may appear injudicious and culpable, if we are to receive the opinions of the older as well as the latest authorities of the obstetric art. If authorities had been the guide for myself in the treatment of these cases, at this early stage of labor, as well as in many others I have seen for several years, I fear I should not have met with so favorable a result as the issue presents, for the mother, as the contingencies of delay would have imperiled the lives of the mothers still more. To obey the *dicta* of those whose views are accredited with Nature's handiwork, would, I believe, as it has been in a great many instances, the silent sacrificial act, the death of both mother and child. I am aware the views I hold conflict with the highest authorities, but I have presumed for many years to differ on this point from them and avoid tradition. Let us hear, to lead off, what my preceptor, Dr. Meigs, says: "The os uteri must be dilated and gone up over the child's head. A man should hardly be justified who inserts his forceps within the os uteri, he must *wait* until the circle can no more be felt." Dr. Bedford says: "To attempt to introduce the blades of the forceps into an undilated os, would in my opinion, be but a passport to the death of the patient, for admitting the possibility of the introduction of the instruments, would not the traction necessary for the delivery of the head be almost followed by the rupture of the uterus?" Dr. H. Miller says: "1. If the whole of the os uteri can be felt, no matter how large the circle may be, or even if the half of it can be easily reached by the fingers, there will be contusion of it by the forceps, and danger of craniotomy.

"2. The head of the child must have descended into the excavation, and the os uteri widely dilated."

Dr. Hodge: "The os uteri should be fully dilated, or at least easily dilated."

Velpeau: "The os uteri should be completely dilated for a greater or less period of time."

Chailly the same.

Cazeaux, my preceptor: "The dilatation or dilatability of the os uteri is even more indispensable here than in the case of version, and we must be cautious that this condition exists."

The three latest authorities on this point are Leischman, Schroeder, and Barnes.

Barnes says: "The cervix must be fully dilated" (p. 89); "also os when three fingers can be introduced, say two and a half inches."

Leischman: "A complete dilatation of the os uteri is indeed, in a sense, absolutely essential, and it is certain that a greater degree of dilatation is necessary for the forceps than for any other of the operations for delivery." He says, agreeing with Ramsbotham, that "there may be cases where it is only one and three-quarters inch dilated."

Schroeder (American edition, 1874, p. 174) remarks: "The cervix uteri must be obliterated, and the os so far dilated that the head can easily pass through it." As Schroeder's views are the latest German opinion on this clinical and practical point, I will state them further: "As regards this condition of the os, the forceps may be applied in an urgent case, as soon as the blades can be passed through the os from an inch and three-quarters to two inches; in case of contracted pelvis, from two and a half to three and a half inches. When the os is as yet little dilated, and while the dilatation continues, it is fixed there, and the progress of the head is retained for a long time; *even then*, there is *no* active interference required, unless the state of the mother demands it. The forceps, in most cases, proves of no avail, and *nothing* more, therefore, remains but to let the child die, and thus facilitate the delivery for the mother by perforation of a dead child."

Dr. W. Goodell, of Philadelphia, in his remarks before the Obstetrical Society, June 26, 1872, says that as a rule, he "waited for the os to dilate, or to be dilatable; but he applied them as a dilating wedge to a rigid os."

Dr. George Johnson, late Master of the Rotunda Maternity Hospital, has been accustomed to apply the instruments when the os uteri is an inch and six-eighths in diameter, for

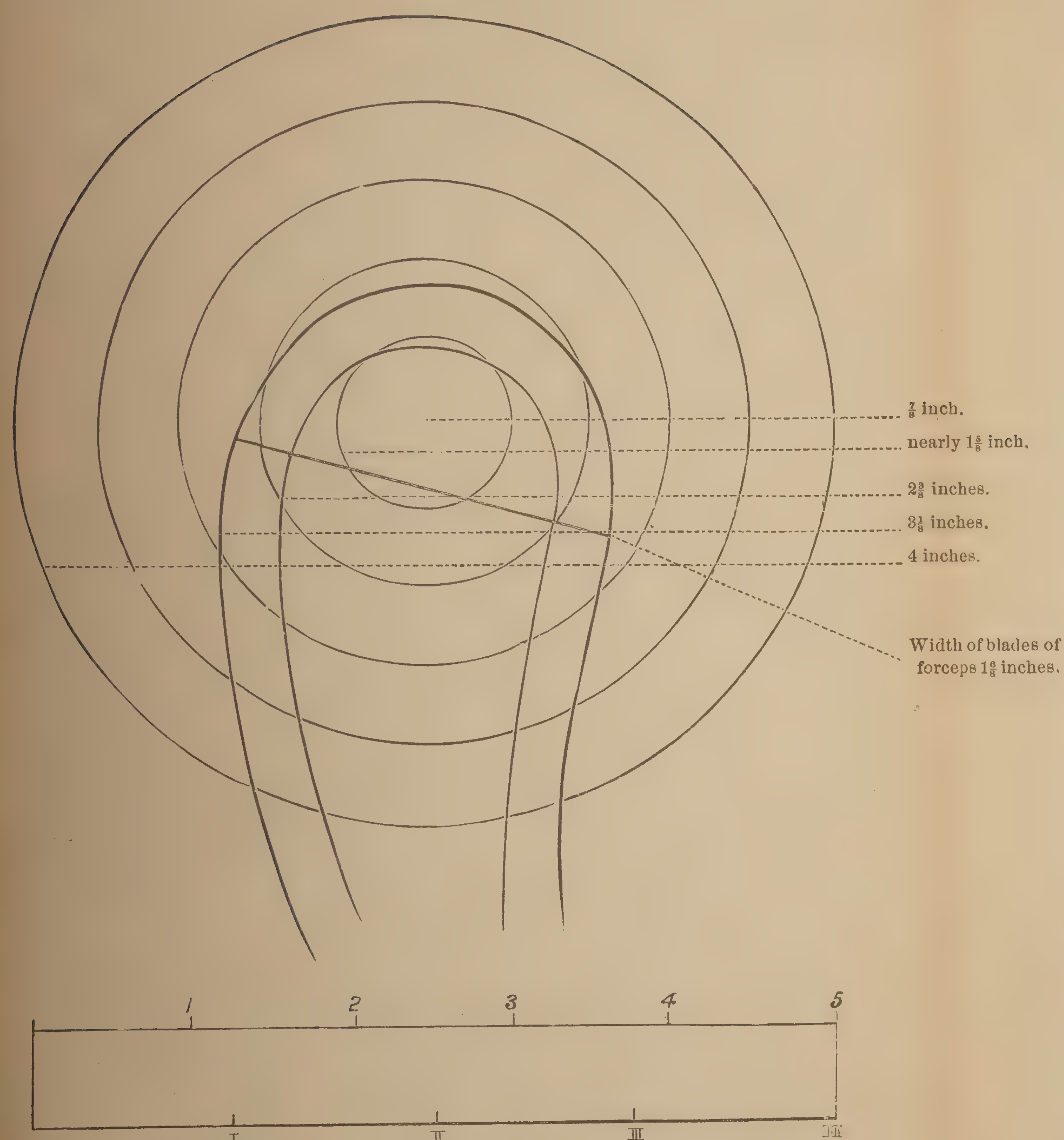
the last four or five years, with the ordinary-sized forceps (Barnes's), and which form of forceps could not be used with safety in the small size of the os uteri as can my own. I present two diagrams, one taken from Dr. Johnson's Sixth Clinical Report, and one of my own, showing the form of the blades of the instruments and the space each requires, and in which it can be introduced. (See Figs, 1 and 2.) I need not enlarge further on this point, with the exception of saying that in some of the cases after the early application of the forceps into the cervix uteri when it has been dilated from one to two inches, the object of the application of the forceps was simply for the instrument to retain the head of the child in contact with the os uteri during and after a pain, and in some cases aid in flexing the head when the vertex presents, so that the occiput may be put in apposition with the os tincæ, and thus become the natural dilator of the cervix.

The simple flat pelvis, non-rickety, as well as the uniformly contracted, are the class of pelves we are more generally called to treat, and they are confined, not to the lower classes of society, but in the higher walks of life. I have, however, met with the obliquely-ovate, the funnel-shaped, and the kyphotic pelvis. I present for inspection to you a pelvis of the simple flat order, and the cranium of a child which was delivered in Bellevue Hospital, by version, and whose case I have related, giving a measurement of two and a half inches in diameter, with indentation; and another child's cranium delivered naturally through a uniformly contracted pelvis of three and a half to three quarters inch.

The contrast in the form and shape of these crania is very decided, giving an illustration of the character of the deformity of the pelvis, which is also very clear and significant as to the method of treatment which we have considered as appropriate.

I believe these kinds of pelves are frequently overlooked, and when the delivery is tedious and slow the labor is classed under the type of tedious labor, especially if it is an occipito-posterior presentation of the child's head, while in reality the cause was a uniformly contracted pelvis of three and a half or three and three-quarters inches in diameter.

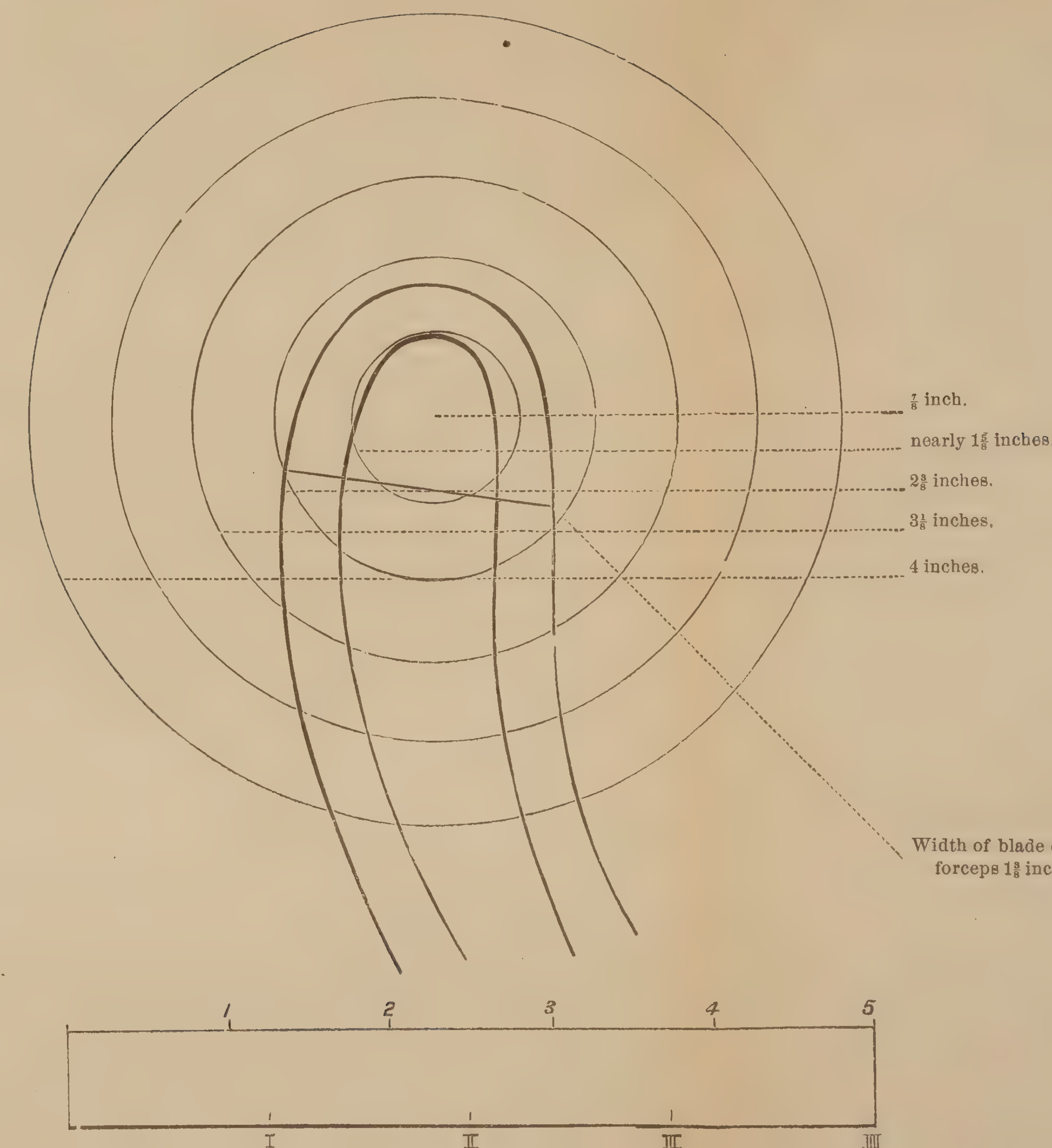
Fig. 1.



Dr. Johnson's Diagram.

From the Sixth Clinical Report of the Rotunda Lying-in Hospital, 1875.

Fig. 2.



Dr. Taylor's Diagram.

Fig. 3.

Small Forceps: Length, 16 inches; greatest width when closed, $2\frac{5}{8}$ inches; breadth of blades, $1\frac{3}{8}$ inch; weight, $1\frac{1}{4}$ pound.

It is not the kind of deformities only we have to deal with, but other considerations should influence our treatment. We are not to be unmindful of the more or less advanced state of gestation, variation in the size of the child's head, its body, male or female, its state of life or death, the presentation, the more or less regular position, the capacity for moulding of the head, owing to its flexibility or a more or less perfect ossification. On the part of the mother a more or less powerful contraction of the uterus, more or less energetic action of expulsion, a variable inclination of the pelvis, sometimes the long axis of the child's body to the long axis of the uterus, or a greater or less amount of laxity of the nervous system, and the time of labor—all these are so many important circumstances which may and do change the result of the labor to such a degree that the indications derived from the pelvic contraction are themselves susceptible of great modification in the kind of treatment, version, or the forceps, and the success of it. It is therefore evident that it is not entirely in the faulty confirmation of the pelvis that the treatment lies, but it is the last one of the elements in the solution of the problem; still there is no doubt the difficulties of labor generally bear relation to the extent of the osseous deformity.

The principal point of importance to consider is, What are the character and formation of the pelvis, which should modify and direct the course of treatment? The profession have been too much inclined to consider simply the contraction, as existing at the antero-posterior diameter of the superior strait, without bearing in mind the presence or absence of contraction in the other portions of the pelvis. It is true the contraction is more generally recognized in the simple flat pelvis than in the generally contracted one, and the treatment incident to both of these is, I believe, as a general rule, entirely different. The pelvis I present to you is a most excellent specimen of the simple flat type, and one rather unusual to meet with — ample at the sides, cavity natural, but decidedly contracted in the antero-posterior diameter of the brim, of only three inches. Such pelves pre-

sent an ample amount of space on either side for the occupancy of the occiput, and present favorable considerations for version, while they may be a bar to the unsuccessful application of the forceps. The deformity in the generally contracted pelvis does not admit of so favorable a result by version as by the forceps. The contraction is not in one point alone, but in all the diameters of the pelvis, and, if version should be accomplished, the space in the cavity of the pelvis is not sufficient or ample enough to allow of but little if any flexion of the head if the vertex presents, to permit the long diameter of the head to be changed to the smaller, the bitrachelo, nor does it admit of rotation taking place so readily should the occiput be posterior, and which I have in several instances noticed in this form of defect of the pelves, more than in the simple flat. Should the contraction of the uterus be powerful, steady, and frequent, the head of the child will be fashioned like the cranium I have shown you, the head being round, and the occipital and frontal bones shelving under the parietal, diminishing the long diameter of the head fully one-half or three-quarters inch in some cases. In a case of labor in this form of pelvis, which I attended in April last, the brow presented, and, after a steady, firm, and energetic action of the uterus, I intentionally permitted it to go on without chloroform for one hour; the head was delivered in that position, with a large caput succedaneum on the brow, and the vertex with the occiput pressed backward on the neck like a tippet. In these brow-cases, if the head does not descend, I prefer making it a face-presentation, instead of the occiput, and they are sometimes very easily delivered. In this case, as the pains were accomplishing the work well and efficiently, I allowed the uterus to terminate the labor without chloroform, as this agent would have only retarded its accomplishment.

The cases of labor in the uniformly contracted pelvis tend more to the necessity and performance of craniotomy, than the simple flat pelvis, for the reasons which I have adduced.

This distinction, in a practical aspect, has not been presented by authors on obstetrics in that clear light which

the nature of the cases demands for treatment. Borinsky has, more than any writer, the last few years, referred to the subject, and holds views analogous to those of myself. Schroeder, although referring to the subject, advocates version as the proper treatment in all cases of contraction of the pelvis. From my own experience, I consider the uniformly contracted pelvis nearly as frequent as the simple flat, non-rickety pelvis.

The treatment by version has, at the present day, assumed a more decided and definite character since the elaborate and able article of Simpson was published in 1847, addressed to the profession, with that energetic spirit he always manifested when writing on any important obstetrical subject. His deductions are, however, drawn more generally and chiefly from the cases related by Lee, Collins, Smellie, Denman, and La Chapelle. His own experience, if we are permitted to judge from the few cases he speaks of, must have been very limited, and he refers solely to the simple flat order. The principal object and purport of his paper were "turning as a substitute for craniotomy, and long forceps, and to avoid the operation of craniotomy," as was customary at that time to resort to. No obstetrician has so clearly defined the practice which should be adopted in cases of contraction of the pelvis, and with considerable experience, than Pugh, of Chelmsford, who, in 1754, states that turning should be substituted for craniotomy, but on this single point Simpson makes no reference to him in his paper.

Pugh's language is very strong, decided, and concise: "Turning in practice is of the utmost importance; it is the grand pillar of midwifery, and operators that are well versed in it will very seldom need the help of instruments. The use of instruments is, however, not to be absolutely rejected as some authors have done, for there are some cases where they are quite necessary." Pugh, although a strong advocate for version in cases of deformity—and he has reference to the same class of cases as Simpson—says that "these are the cases where numbers of children lose their lives, and many women. The grand objection to turning with all operators is, that

the narrowness of the passages through the pelvis, which hindered the natural expulsion with the head foremost, will hinder its extraction when brought forth by the feet, so that no other method but that of opening the head has been hitherto practised; this certainly destroys the infant, but turning will remove the difficulty without proceeding to so dreadful an operation." The practice of Pugh did not receive the attention which the subject should have claimed, for we find it not recognized from his day till, if I may say so, it was exhumed by Simpson, and brought out afresh in 1847. From the time of Pugh till 1847 the forceps were resorted to, and that to a limited extent by the English authorities, and afterward the short-handle forceps appear to have taken the precedence of the long during the time of Denman, Osborne and some others. Burns was nevertheless a strong advocate for the long forceps, and scarcely refers to version in these cases. He was a decided opponent to the *laissez faire* or wait-system of treatment carried out by Denman, Osborne, and others. It was especially recommended by Madame La Chapelle and by a few Germans. As a method of treatment it was rejected in France, and as W. Jones says, "without having been sufficiently tested," though suggested by Baudelocque, who considered that the structures of the head of the child are such that it collapses more easily in its width and enters more readily when the child comes by the feet, if it be well directed, than when the head comes first. Cazeaux, in his last edition, some four years since, had changed his method of treatment materially in favor of version from the use of the forceps in pelves ranging from two and three-quarters to three and a half inches. The principle enunciated by Ould was precisely the same as that of Pugh, and in its explanation he admits he had no practical experience of the fact. Simpson, explaining the advantages of version, considered the head of the child in its *ensemble* as a truncated cone, having for its upper base the biparietal diameter of three and a half to three and three-quarters inches, and for its lower base the bimastoid, two and a half to three and a quarter. The latter, the bimastoid representing the base of the cranium, is a solid

osseous mass, and therefore irreducible, while the former is elastic and perfectly reducible to the extent of from one-third to one half, for, owing to the presence of the sutures and fontenelles, we must remember the base will be increased in proportion to the size of the child's head, and this is the principal point of the difficulty to be overcome in the direction of the case, whether by version or the forceps, or naturally, I may add, for Nature has her own beneficent influence, and teaches us sometimes how we should imitate her method of delivery.

The explanation which Simpson has presented has not been recognized as being entirely correct, and other explanations are deemed more accurate in aiding to solve the mechanism of the delivery of the child's head, in cases of contraction of the pelvis, after the delivery of the body, or when the head presents. Hodge's objection has its practical truthfulness, when he says that "the base of the cone of the head should be located at its superior part, the rear base being at the occipital extremity, and its apex at the chin and not at all at the base of the cranium." As bearing on this view of the subject, and as it has, in some measure, aided me in the management of difficult cases after version, and the direction of the head through the superior strait, I will refer to a case I saw with Dr. J. Sidney Crane, many years ago, among several others of a like nature, where the pelvis was slightly contracted at the brim, about three and three-quarters in the conjugate. In two successive labors, the forceps were obliged to be resorted to. In the last labor I saw her with Dr. Crane, and as the labor had continued for several hours, the vertex presenting, and no advance toward delivery, she was delivered with the forceps. In the third labor I was requested to take charge of her. When her labor commenced I was summoned, and every preparation made for a like event as before. I saw her two hours after the commencement of her pains. On examination I recognized the head presenting in a perfectly flexed condition, the occiput dipping, as clear and distinct into and through a cervix about half opened, and the head as plumb as it could

possibly be, truly synclitically, in the right oblique diameter; membranes intact; uterine forces strong and efficient; cervix thin and dilatable. I tapped the membrane during a pain; the head descended through the cervix, and the child was born in a few minutes after; the child weighed eight pounds, which was as much as her former children.

An illustration of opposite character may be pertinent to the subject. In some instances where the deformity has been slight, and the vertex (not the occiput) presented, the frontal bone somewhat lower than the posterior, and the application of the forceps has not succeeded in removing the difficulty, I have made the vertex, either by the finger on the brow, or the right-angle blunt-hook on the chin, a face-presentation, the forceps was then applied over the transverse or smaller diameter of the head, three and a half inches, instead of as before over the long oblique of four and a half inches.

A very instructive case of this nature was operated on in 1870, in the presence of Dr. Baldwin, President of the National Medical Association, and three or four other delegates, with success. These illustrations by Nature and by art, changing the relation of the head in its diameter from the longer to the smaller diameter in some cases, tend to militate against the absolute indications which Pugh and Simpson have enunciated.

The advocates of version assert as favorable for this operation—

That the head is placed transversely; is caught in its smallest diameter, the bitemporal, and that the osseous base is the first to enter;

That the bones bend in and overlap better, and that it is half an inch less than the biparietal;

That the cord can be well and safely protected;

That the child can bear very great strain with impunity, and conjoined with propulsion from above to the extent of two hundred pounds;

That, when version fails, the operation of cephalotripsy or craniotomy is no more difficult than when the head presents.

The objections to the forceps are—

That they injure the structures of the parent, and the head of the child;

That they occupy the fronto-occipital diameter of the head, the head being transverse;

That, when compression is made, the diameter of the bi-parietal will be increased or bulged out, and these prevent the descent of the head of the child;

That they are merely tractors, and that, if compression is made, the compression cannot be but very little;

That the forceps cannot be applied as early for delivery as turning;

That the cord may be pressed by one blade of the forceps on the medulla oblongata.

From some of these objections against the forceps I must dissent, while believing as I do in the great advantage of version in some cases.

In relation to the forceps being placed fronto-occipital, if the long-curved forceps are used, this will happen; but, as a general rule, though the head of the child is obliquely transverse, there is ample space for the blades of the instrument to be applied, and they will then more usually take the oblique position—one on the occipito-mastoid, and the other on the fronto-orbicular—whichever position the head may be located in the pelvis, right or left.

If we select, however, the long, straight forceps, the head will be grasped more in the transverse, with one blade near or over the occipito-parietal, and the other fronto-parietal, as they cannot be applied directly antero-posterior.

That compression made transversely we all admit will not impart as much injury to the child's head, and that it is safer than propulsion from turning when much force is used;

That the mother's structures are not suffering, as the compression is to be made, and firmly made, on the child's head, and thus in some measure relieving them from pressure;

That compression may be made to the extent of half an inch, this being the limit of the compression by the instru-

ments as well as in version, unless the head is very flexible, and as much as we should desire in the use of them, if the diameter ranges as low as three and a half inches;

That full, firm, and decided compression is justifiable, as there can be no more injury done than we witness in the moulding process by Nature when the head has been compressed as small as two and a half inches, and living children the result.

I will concede that the compressive power of the pelvis, through strong and expulsive action on the child's head, and its flexibility in many cases, is entirely different from that of the forceps, which is only in one direction, as it is in version from the compression made by the promontory of the sacrum on the infant's cranium.

That the reason which is urged in favor of version being earlier performed, is incorrect, if the ordinary rules for version are adopted: that the cervix should be opened to the extent of two inches to admit the hand to effect turning, will be amply sufficient for the application of the forceps, even if the os uteri is as small in diameter as six or seven-eighths of an inch.

But the hand need not be introduced, for by external manipulation and two fingers it may and will succeed.

Dr. Milne, of Edinburgh, who is a strong advocate for turning in cases of contracted pelvis, and still more so for premature labor, remarks, respecting the use of the forceps in the high operations when the head is at the brim: "If the head will not come down without great force in such cases, necessitating great compression of the cranium, this fact alone is damning to the high operation. The long forceps are of necessity damaging to the mother and child, and when unjustifiably undertaken will haunt the operator when awake or asleep. The forceps ought not to be used as a compressor, but a tractor; when you employ it to squeeze and crush the head you prostitute it entirely. It is true you may with a considerable fight extract a living child, although reverses often happen. But, what of the future history, not to speak of maternal injury, to which we shall refer?"

"As regards the long-forceps children—I mean their his

tory—not a few practitioners have noticed their merging into idiocy from the compression being great antero-posteriorly." In the practical remarks I shall make on the subject I shall, from a long and extensive experience, be constrained to take conservative ground, though an advocate of such procedure in cases we are considering.

If this gentleman, before he had passed such denunciations on the conservative use of the long forceps and high operations, had consulted Hecker and Weber on the subject of the future condition of the infant consequent on labor, he could not have attributed to the instruments the unfortunate circumstances he has.

The dissections of these gentlemen show the important fact that mechanical injury of the fœtal head, neck, or trunk, is not necessary for the production of intense congestion and blood-extravasation of the serous membranes of the brain, spinal cord, and chest. Ollivier has observed especially, and others maintain, that while at natural birth the spinal cord is perfectly developed, the brain is still in a very rudimentary state, and consequently able to bear considerable disturbance without ultimate injury to its function. In fact, in the new-born child, brain-life is entirely absent. Dr. J. Crichton Brown, in 1860, attributed idiocy sometimes to difficult labor solely.

With the strong denunciation against the long forceps, curved or straight, I am compelled to assert that, having in a very large number of cases operated with the head at or above the superior strait, in public and private practice, I have yet to realize the unfavorable condition, from their application, referred to; and yet extraordinary and great force, which some consider imperative, I have not resorted to. In version some force is called for, not only on the body of the child, but the head, by pressure from above the pubes. Has not great force been used on the body of the child, even to the very limit of human traction on the neck of the child, so that the tissues have been heard to crack and snap with the force employed? Is this more mild in treatment than the compression of the forceps

and its results as supposed? But are not both these methods justifiable? Can we in the delicate position we are placed in; in cases of this kind, with the head of the child grasped in one instance by the blades of the forceps, or the after-coming head nipped and compressed by the promontory of the sacrum, can we hesitate to use that measure of force which may be required, tempered with judicious action as to our object and experience from the nature of the case—or shall we wait for the child to die or the employment of craniotomy? In judicious hands I believe it to be perfectly correct and right to do so, for success may crown our efforts. But in the failure we must adopt the alternative, and prepare for the sacrificial act, and save the mother.

Let us canvass some of the facts presented to us by a few authorities, and among these the latest, even if they have been referred to by others:

Of the seventeen children reported by McClintock, of Dublin, delivered by version, nine died.

Of the nineteen cases of Madame La Chapelle, five belong to the class of slight contraction in which all the children were saved, one mother only dying of peritonitis.

In ten cases of considerable contraction all the children died, and four mothers died.

Of the nine cases of Dr. W. Goodell, of Philadelphia, eight children were living and one died. A most excellent success under the course of management adopted, with powerful traction and propulsion from above the pubes on the head of the child.

In Jones's reported cases, of fifty-one in contracted pelves ranging from four to three and a half inches, delivered by the forceps, fully one-half of the children died, while, in McClintock's forty-seven by version, nearly all the cases were slightly contracted at the superior strait, being the simple flat pelvis, and most of the women had given birth to their children by natural efforts before.

Nevertheless, in every instance great force was employed subsequently to turning. No reference is made by Dr.

McClintock to propulsion from above the pubes to aid the delivery. In one case powerful force was used in order to bring the head into the cavity of the pelvis. Dr. McClintock admits he is not a strong advocate for version, and that he is no expert operator with the long forceps. He thinks the good opinion of the method by version has been exaggerated by its advocates, and that it is only in slightly contracted pelves that it can be resorted to with any chance of success. Dr. Goodell's results are the most favorable of all the authors with whom I am acquainted. With the amount of traction, with the propulsion which was used, and the success attending it (although there were much cracking and snapping of the tissues of the child's neck as he says), we must admit the children made a narrow escape of their lives.

We are all aware that the force of traction upon a child's neck may be considerable in some instances, and that a living child's cervical structures are much stronger than a dead one's. Should the remark by the Edinburgh accoucheur, that "the forceps may be considered as the cause of producing, by the compression of the blades, idiocy," and which I have referred to as having very little, if any, cause in establishing it, be brought as an argument against the use of the long forceps in the superior strait, and especially in deformities?

We are not, however, to forget that in version the danger is equally as imminent, if not more so in some respects, for the welfare of the child. Joerg asserts that immediate death, although the heart continues to pulsate for several minutes after birth, follows mechanical injury, such as stretching or twisting of the cervical vertebræ. It is true that children may recover from considerable injury in this situation. We recognize also, from positive investigation after death, that the future existence of the child may show affections of the glottis, pharynx, tongue, lips, spastic rigidity of the limbs in the consequent arrested development of some of these parts, and these results may be referred to injury at the base of the brain and medulla oblongata. Little has observed more than fifty cases of injury, of mind and body, from abnormal parturition,

the subsequent symptoms indicating that the brain and medulla oblongata had permanently suffered, and the only one of the nervous centres which presented symptoms of lesion was the medulla oblongata.

Looking at the various reasons for the employment either of version or the forceps, for the benefit and welfare of the mother and child, considering the contingencies attending each method, either of them being elective operations, our special object should be, if possible, to avoid perforation of the cranium of the child. It is certainly very difficult to decide upon which we shall elect, and we must be guided by the peculiar circumstance of each case which may be presented for our counsel. In the simple flat, non-rickety pelvis, version may be preferable, but it is not as available, I believe, as a general rule, in the uniformly contracted one. The powerful traction and propulsion which were exercised in some of the cases of Dr. W. Goodell were certainly very great. I shall refer to case No. 8, a uniformly contracted pelvis, as one of the typical cases for this method. Dr. Goodell says that Dr. Roberts and himself together exerted a force of not less than two hundred pounds; in Case IX., simple flat pelvis, of two and four-fifths inches, a force of one hundred and fifty pounds.

My friend Dr. Goodell says that exerting all the manual strength he could command, he has never seen the body part from the head. I have unfortunately seen three instances, although not the operator, and the children had been dead but a few minutes.

In this class of labors, to uniting propulsion from above the pubes with traction on the body and neck of the child, Dr. Goodell considers his success was to be attributed. He remarks that, "so far as I can judge from the history of the cases from Simpson, of the delivery of a child in a diameter of 2.5 inches, and which was dead; of Schroeder's case, of a living child, through a diameter of 2.8 inches; of Blot's case, of three inches, and Taylor's case" (my own) of $2\frac{3}{4}$, as he has it, but which should be $2\frac{1}{2}$ inches; "in none of these cases did the operators, excepting myself, invoke the very sub-

stantial help of a propelling force, and hence the inference is logical, that the conjunction of traction and propulsion offers better results than version in these cases."

In reply to this remark of Dr. Goodell, I must say that I have for fully fifteen years adopted this double method of treatment in these cases, and performed it several times before large classes of students at the Bellevue and Charity Hospitals. At the Women's Infirmary the same course has been witnessed by many of my friends. The specimen of the head I present to you this evening, and which Dr. Goodell refers to, was delivered in this way before a packed audience in a very large amphitheatre at the Bellevue Hospital. The head was not fractured at the time of delivery, but the fracture occurred from a fall afterward. The bones were bent in, and the specimen is one of the most perfect illustrations of the kettle-drum indentation you can find.

The indentation is more anterior to the fronto-parietal suture. It also demonstrates that there was no compression of the parietal plates by the sides of the pelvis, as they are as wide as when delivered naturally, head first. This practice I claim no originality for. Pugh, as I stated, was the able advocate for version, and on this point he records his opinion as follows (Pugh's "Midwifery," 1754, page 53): "After the body is delivered, keep your left hand on the neck still in its place, never let that go; direct the nurse, or one of the most handy women about you, to get upon the bed, kneeling close by the side of your patient, with her face to you, and put her hand under the bedclothes, close to your patient's pubes, with the inner part of her arms turned to your patient's body; then with your right hand feel externally for the child's head, and the most proper place for it, not exactly over the pubes, but on one side toward the groin; then fix the hind-part of the palms of both her hands upon the child's head, bidding her press down *pretty* strongly, you pulling the child at the same time, and by this method, and with such assistance, I have never once failed of success." He says still further: "The child's head is capable of being

moulded into many shapes, as the bones are flexible, and will admit of being bent a great deal without any injury, or very little."

I have not, in my operations on the after-coming head, used the great amount of propulsion, or of traction on the neck of the child that my friend Dr. Goodell has, nor have I ever applied to forceps operations the extraordinary force that has been described by some writers. I believe that it is absolutely necessary, in the management of this class of labors, that we should, if version is adopted, try if possible to adapt the head to the largest part of the pelvis, which is the sacro-iliac space. Examining the cranium of the child I have shown this evening, it will be perceived that there is no compression of the parietal bones, and the head is as large and as natural, except the indentation. It must be apparent that, if a cranium of that size could be delivered through a pelvis with a sacro-iliac space as large as that, though the conjugate was only two and a half inches, it is probable that if some other method of managing the head could be adopted it might succeed. There are objections to the manner of delivering the after-coming head in these cases in the ordinary way. The head as I have found has been placed slightly obliquely transverse and not directly transverse, as is usually considered. The smaller diameter of the head, the bitemporal, rests upon the narrow part of the pelvis, the conjugate. If traction, therefore, by one or two fingers inserted into the mouth of the child is made, the superior malar bone would be made the larger part of the head, and would be brought in apposition with the contraction and the head could not be delivered. The first step to be carried out or adopted is, after the body is born, and the head is brought into the proper position and relation to the pelvis, the smaller diameter, the bitemporal, is made to correspond with the contraction. The body is carried backward, and the head is to be pressed downward and backward toward the cavity of the pelvis with propulsion from above the pubes in the same direction as the body. Should it be found to be advancing ever so little and yet retaining its position, then by bringing the body of

the child quickly forward toward the pubes and making firm and decided pressure on the head downward and forward in the axis of the outlet, it will sometimes pass very easily and quickly into the cavity and be soon delivered.

Another method may be adopted, which has sometimes succeeded in my hands, and where the diameter has been as small as three to two and a half inches, if the first method has not succeeded, and there appears more delay than we had anticipated, and to avoid craniotomy, lift the body of the child as much anteriorly as it will properly admit of, then with the assistance from above direct the posterior part of the child's head to the sacro-iliac space, and press with some force on the forehead of the child, the face looking upward. I will illustrate this method by the recital of a case which occurred at the Woman's Infirmary. The patient had a simple flat pelvis, with a contraction of three inches; she was eight months and one week advanced in pregnancy. She had been delivered once before by craniotomy a few years since. Premature labor was induced by Lazarewitch's method. When the os uteri was two inches in diameter, external version was made and the two-finger turning adopted. The body of the child was delivered intentionally with the back looking downward, face upward, occiput to the left sacro-iliac space. With the valuable assistance of my friends Drs. Burrall and Ward, propulsion was made by pressing the posterior part of the head into the sacro-iliac space, then downward and backward, and traction forward in the axis of the outlet by having the body carried upward. In a few minutes the child was delivered in a semi-asphyxiated state, and by careful and persistent attention was restored. Child weighed over eight pounds. This patient, eighteen months after, entered as my private patient in the Bellevue Hospital, at the eighth month of pregnancy, and premature labor was induced by the water-bags, and she was delivered before the class in the course of an hour by the same method. Child living, and weighed over seven pounds.

The application of this manner of delivery is sanctioned,

as a general rule, in the cases of the obliquely-ovate pelvis, by adapting the posterior part of the child's head to the largest part of the pelvis. In my second paper I shall have more to say on this method, as applicable to more diminished pelves. Dr. Goodell carries out the same plan I first stated, only he reverses the direction of the body of the child, I believe, by bringing it first in the direction of the outlet and swinging the body backward on the perinæum, accompanied by propulsion from above. Should this method not succeed, he tries the to-and-fro movement, or what he styles the "pump-handle plan." The swinging, to-and-fro movement is especially resorted to by some obstetricians in the delivery of the child by the forceps, if gentle traction is not successful. The side-to-side manipulation, when the forceps are applied transversely, not antero-posteriorly, on the child's head at the superior strait, is necessary for tilting or canting the head in some cases, and will succeed in delivering the child, when strong pressure and traction fail; for, the tilting or obliquely delivering the head, independent of the pressure from above and the traction from below, is the great principle to effect the delivery of the child's head from its "durance vile," in these cases.

I pass from the treatment of version, to offer a few remarks on the forceps.

As I do not find that the head of the child in these cases is placed, as it is generally believed to be, always directly transverse, but slightly obliquely transverse, there is sufficient room for the application of the *long*, *straight* forceps, not the *curved*, with one blade in the sacro-iliac field posteriorly, and over the side of the occipital bone, and the other in front of the acetabula and over the fronto-parietal suture, or the side of the frontal bone. If the long, curved forceps are used, the head will be seized either slightly oblique, or directly over the frontal and occipito-parietal bones—in truth, directly on the long diameter of the child's head. As an illustration of the value of the long, straight forceps over the curved, I will cite the following case, among others, of their beneficial use, and

which induced me to give them the preference in these cases over the long curved ones.

E. T., aged twenty-eight years, came into the Bellevue Hospital May 4, 1867, at 6 P.M. The os uteri, on examination, admitted the finger. At 12 M., May 5th, the os was fully dilated. At 2 P.M., membranes ruptured. Dr. G. T. Elliot saw her at 8.30 P.M. Head in the left occipito-iliac transverse position. Deformity of the pelvis recognized. Supposing that the head was advancing, he left the patient, and I was sent for at 7 A.M., May 6th, and saw her with him at 8.15 A.M. Caput increased. No advance of the head. No impaction. Head readily moved above the brim, and the operation of version quite feasible, and elective. Fœtal heart audible. We decided, after consultation, to deliver with forceps. The first blade was readily applied in front of the left sacro-iliac synchondrosis, the second blade behind the right acetabulum. My friend and colleague was accustomed to use his forceps with a movable slide, something like Mende's, so as not to compress the head of the child. Strong efforts were made with his curved forceps as tractors, but without effect. Craniotomy being suggested by him, the long, straight forceps were requested to be made trial of. Application easy. Compression made, and traction directly backward and downward on the perinæum. In a few moments the child was delivered. Efforts to restore the child were ineffectual. Diameter, three and a half inches. Child, seven pounds. I am an advocate for version, considering all the arguments in favor of the principle, as I believe it will have to be adopted in many cases, and take the place of the forceps if they fail. If we accept the statement of some authorities, living children have been delivered through pelves of two and three-quarters to three inches diameter by the forceps. In this statement, however, no mention has been made of the size of the child, the sex, or the flexibility of the head, or the moulding of it, and therefore we cannot form a proper estimate of their application in pelves ranging from two and one-quarter to three and one-half inches. The success of such cases is in the

opinion of various authors the exception to the general rule. Meigs, Hodge, Dewees, Ramsbotham, Churchill, Gardien, Boivin, Chailly, Velpeau, have referred to such cases, but they give little or no personal experience themselves. It is a difficult task to deliver the head of a child at the superior strait at full term through a pelvis of three and a half inches, even with the long, straight forceps, unless special circumstances favor it; but it is an extremely difficult work when it occurs in a generally contracted one. I think it better in some cases, for the reasons I have adduced, to adopt that course with firm compression, than it is to resort to version, and then almost certainly have to perforate. Meigs gives no sanction to the compression of the head, and he met with very few cases of deformed pelvis. Hodge is a strong advocate for the forceps, and believes in acting with firm and decided compression. Hour after hour has been spent by some in making trial of the forceps, and no success has followed the effort. On such treatment as that, comment is deemed unnecessary. In none of the cases reported, where the forceps have been tried in cases of contracted pelves, has the different kind of pelvis been mentioned. We cannot, therefore, draw or form any estimate of the valuable resources of version, which must take the precedence of either form of forceps, for the forceps are considered as ranging in their usefulness in pelves from three and one-quarter to four and one-quarter inches, and seldom are applicable even in pelves of three inches. The compression of the child's head, if it is well ossified, will be very small; but when the flat bones are thinner and soft, the fontanelles large, and the head small, then it can be delivered. In some cases the head is small and sufficiently moulded by the forceps, but the shoulders and body of the child may be so large, and the traction-force required so considerable, as not to succeed. With version, in cases of this nature, the traction on the body and the propulsion from above the pubes are more available. In some cases I have succeeded with version after I have failed with the forceps, and reversely. I am always unwilling to continue with the forceps—whether at the superior strait, or in

the cavity of the pelvis—very long at a time, or with very strong efforts, believing that a withdrawal of the instrument and its reapplication in a few minutes are best, even if it is three or four times applied. Permitting the uterus after each application to aid in altering its position some, I have, after a while, been able to deliver the child by merely lifting the instrument in the proper axis of the pelvis with two fingers. In a contracted pelvis—the simple flat pelvis—if I have not been fortunate in changing the location of the head, and there is no prospect of delivery in half an hour, I would proceed to version. In the uniformly contracted pelvis, the forceps are, I believe, the proper remedy; and if, after a short time, success is not obtained, which it is in many cases, it is my firm conviction, as the occiput is more generally posterior in these cases than in others, embryotomy will have to be performed.

A slight reference to the practice of the French would not be amiss at this moment. The practice adopted by P. Dubois, Depaul, Pajot, and Blot, in cases of moderate contraction is, after waiting till the complete dilatation of the os, to apply the forceps and make moderate traction. If this proves insufficient, the instruments are to be removed, and not applied again for two hours. If no result be obtained, and if a second or third application be fruitless, recourse should be had to embryotomy; version is seldom entertained.

To avert the operation of embryotomy has been the special purpose of all these investigations and experiences, and, as I remarked at the commencement of my paper, it is our duty in every way to try to save both mother and child; but, if we cannot save the child, by all means the mother.

In cases of this character the accoucheur's conscience and responsibility are very greatly at stake. He is unable to lay down any immutable regulations for his guidance. Is he to act, or is he to forbear? If he delays, the child will be sacrificed. If he acts by either method, the life of the child is in great jeopardy, and he must be neither too rash nor too dilatory; for, by delay, many a valuable parent's life has been lost. Some obstetricians only wait for the spontaneous

death of the fœtus, before they deem it right to interfere. This course of treatment has been advised by some prominent accoucheurs, and by one of comparatively recent date. The *laissez-faire* treatment of Denman, Osborne, and others, who were decided representatives of the procrastination treatment, and who wait for Nature to assert her prerogative, has, I believe, swept many a mother from existence.

PART II.

IS CRANIOTOMY, CEPHALOTRIPSY, OR CRANIOCLASM, PREFERABLE TO THE CÆSAREAN SECTION IN PELVES RANGING FROM ONE AND A HALF TO TWO AND A HALF INCHES?

Read March 2, 1876.

THE obligations I am under to the Section on Obstetrics of this Academy, to prepare a paper on the question "What is the Best Method of Treatment in a Case of Labor with Contracted Pelvis?" are now to be fulfilled. This question I have considered as a general one, and therefore have thought proper to divide it into two parts.

The first part I read September 16, 1875, before this Academy of Medicine, and in it confined my remarks to the deformities ranging from two and a half to three and three-quarters inches antero-posterior. In that paper I particularly referred to two kinds of deformities which we are every now and then meeting with in private as well as in public practice, and the treatment applicable to each. Dwelling upon the two different deformities, I showed that in the generally contracted pelvis, when the diameter ranges from three and a quarter to three and three-quarters inches, and the simple flat pelvis, ranging from two and three-quarters to three and three-quarters inches,

the treatment ought to be entirely different. In the simple flat pelvis, where the application of the forceps after a short attempt had failed, *version* would be the most appropriate method to adopt, under the expectation that by traction on the body of the child after version, with propulsion from above the pubes, the child might and could be delivered without embryotomy, while in the generally contracted pelvis, should *version* be adopted, *embryotomy* would have to be performed in some instances. I also stated that in the simple flat pelvis the long, *straight* forceps would be preferable to the long, curved ones. In the generally contracted pelvis the curved forceps are the most efficient and suitable instruments. I presented two specimens of infants' crania, illustrating the form of the head and the moulding of these heads consequent on the different formation of the pelvis, and demonstrating the character or type of the pelvis. The cranium which was delivered naturally through the generally contracted pelvis was round, caused by the overlapping of the frontal and occipital bones, by the parietal diminishing the *long diameter* of the child's head fully one-half to three-quarters of an inch. In the other cranium, delivered through a simple flat pelvis of two and a half inches antero-posterior, by version and propulsion from above the pubes, the head was flattened or indented at the junction of the parietal and frontal bones, which is the usual place. The dimensions of the child's head in all the other diameters were of the ordinary measurements of a child at full term.

The *second part* of my paper I enter upon this evening, and shall consider the question, "*Is craniotomy, cephalotripsy, or cranioclasm, preferable to the Cæsarean section in pelves deformed from one and a half to two and a half inches?*" and present the history of a case of labor in a kyphotic pelvis of one and seven-eighths inch diameter at the inferior strait.

The question is one of considerable moment, to which the attention of the accoucheur should be invited, involving a highly responsible position. We are all aware that, in natural labor,

the accoucheur has little else to do than to watch his patient, and patiently await the termination. In obstructive labors, from any cause, and especially in deformed pelves, no matter whether the pelvis is ever so slightly diminished in its antero-posterior diameter, or of a slight generally contracted formation, difficulties soon manifest themselves, and a series of symptoms spring up, which require all his wisdom to secure the safety of his patient, as a double responsibility rests upon him, for two lives are involved. Under such trying circumstances the accoucheur will have great cause for congratulation if, by his most judicious efforts, he is able to preserve the most valuable life—*that of the mother.* In the whole range of surgical and obstetrical practice, there is no occasion which calls for more deliberation, more precise knowledge, and a clearer judgment, than are required to enable the obstetrician to determine in difficult cases of labor, in deformed pelves, how long he may trust to Nature without compromising the life of the mother, or entailing upon her an existence of misery worse than death itself; or, on the other hand, to fix the precise time when he is unfortunately importuned to sacrifice the life of the child for that of the mother, or, by the performance of the Cæsarean section, perchance save the child, and possibly the mother. The several operations—craniotomy, cephalotripsy, cranioclasm, or the Cæsarean section—have their unfavorable and unfortunate aspects, and the decision upon the peculiarities or circumstances of the case must rest with the judgment and experience of the operator.

In the British Isles the profession are generally, though with few exceptions, adverse to the Cæsarean section. Radford was strongly in favor of it, especially if early performed, even if the "blackness of the account," as he styles it, of the mortality consequent upon the operation was so great. At the time he wrote, in 1848, the record was, out of forty-nine women who had been delivered by the Cæsarean section, only four were saved.

Greenhalgh's (of London) experience has determined him, for the future, never again to attempt delivery "*per vias natu-*

rales," unless there is fully two inches in some part of the antero-posterior diameter of the brim. He says that craniotomy and extraction by the crotchet or cephalotribe, in cases of extreme deformity of the pelvis, are more difficult and probably more fatal to the patient than the Cæsarean section.

P. Dubois believes that, "when the contraction of the pelvis approaches two or even two and one-fourth inches, recourse must be had to the Cæsarean section." De Paul entertains the same opinion. The Germans, taking as an exponent Schroeder on the subject, consider 2.1 as the extreme limit of craniotomy, and the Cæsarean section is, therefore, the sole means of delivery.

Schroeder, however, remarks that, "as there is a possibility of another way of delivery than the Cæsarean section in the highest degree of pelvic contraction, it may be necessary to perform craniotomy, whether the child be alive or dead."

Cazeaux observes that, "when the pelvis offers at least two inches in its smallest diameter, embryotomy must be performed, even if the child is alive."

Leischman remarks that, "as ample proof has been adduced that craniotomy has been successfully performed in contraction of one and three-fourths inch, and that it may be in one and a half, when the conjugate diameter exceeds this limit, we are in no case justified in at once deciding in favor of the Cæsarean section."

Barnes declares that "it is perfectly unjustifiable to neglect embryotomy, and to cast the woman's life upon the slender chances offered by the Cæsarean section." Out of

480	operations in	England	236	recovered,	244	died.	Ratio,	50	per cent.
712	"	Germany	332	"	380	"	"	53	"
344	"	France	153	"	191	"	"	55	"
11	"	Belgium	4	"	7	"	"	63	"
46	"	Italy	5	"	41	"	"	87	"
12	"	America	4	"	4	"	"	33	"

The general average of these records would give a mortality of fifty-three per cent. Let us, however, from this exceedingly unfavorable presentation of mortality, turn to that of Dr.

Harris, which is the latest record. In his paper published in the February number, for 1872, of the *Obstetrical Journal*, seventeen operations are recorded; out of these $73\frac{10}{13}$ per cent. were operated on the first day and recovered, and $26\frac{3}{10}$ per cent. died; $86\frac{2}{13}$ per cent. of the children were saved by operating early.

Dufiellay, of France, analyzed all the cases which had been operated on between 1845 and 1861. Where the woman had been operated on early, before the strength of the patient was exhausted, three-fourths, or eighty-one per cent., recovered; where marked symptoms of exhaustion were manifest, only nineteen per cent. were successful. Cazeaux had previously shown that, when the operation is early performed, it was more successful. Keyser's record, nevertheless, states that when the operation was performed after the labor had lasted

24 hours there were	20	recovered,	$40\frac{2}{3}$	died.
24 to 72 hours	34	"	$41\frac{1}{3}$	"
over 72 hours	8	"	$21\frac{1}{3}$	"
	62		$103\frac{1}{3}$	

Taking the same time as to the life of the child, out of one hundred and fifty-eight cases operated on

After 24	hours,	42	living,	16	died.	
From 24 to 72	"	48	"	24	"	
After the 72	"	11	"	17	"	
		101		57		Nearly one-half died.

This is also the opinion of Radford. Keyser's opinion does not, with his showing, aid in sustaining the views of Harris and others as to the earlier the operation the more successful the result. It will be conceded, when we review the cases of Cæsarean section, when performed in the country, a more favorable result would be more likely to occur than in the large cities or hospitals. It has been asserted by the French provincial surgeons that three-fourths or four-fifths of the women who underwent the Cæsarean section recovered. If so, this would tend to attest the views of Harris and others,

and therefore in patients operated on in the country the result would be more successful than in the city.

Pahan Dufiellay's favorable opinion of the Cæsarean section does not, however, seem to have met with the confidence of all the obstetricians, some calling in question the method of his statistics.

If the statistics of Dufiellay and Harris are recognized as bearing on the question as to the propriety and absolute necessity in extreme cases of deformity from whatever cause (for it is not only an operation of forced necessity, but one of election), we should only consider it as indicating an approximation to a probable mortality. Still, they tend to show that not only the Cæsarean section, but embryotomy, and all capital operations, as ovariotomy, when performed early, especially in the country, would present a more favorable evidence of success. It is a very difficult task to present a comparative estimate between the operation of Cæsarean section and craniotomy, for the operation of embryotomy is called for in cases of emergency, as in convulsions, hæmorrhages, and other contingent circumstances, and certainly should not be classed in a comparative investigation. If a comparative examination, between the two operations, in contracted pelves of one and three-fourths to two and one-half inches, of the antero-posterior, or transversely in the inferior strait, have been recorded, I am not aware of it.

Hicks and Phillips have shown in vol. xiii., 1872, of the "Obstetrical Transactions of London," that the cases collected from Lee, Churchill, McClintock, Collins, and Ramsbotham, and some others, forming the statistics from the records of craniotomy are unworthy of confidence, and that there are but *few instances* where the operator is *accountable* for the result.

No one will deny that the danger increases more and more rapidly where the smaller diameter or contraction is apparent, through which a mutilated fœtus is to be dragged in those cases, than where the contraction is not as great.

The cases requiring instruments, until very lately, were

delivered chiefly by the craniotomy forceps, or the crotchet after perforation, principally the *crotchet*, and generally after long delay.

The success of craniotomy is very much influenced by the period at which the operation is performed; by the presence or absence of complications; by the previous attempts, and often repeated, at delivery; by the condition of health of the mother at the time of the operation; and equally so by the choice of instruments, the manner and tact of operating, the full comprehension of the relations of the child's head to the pelvis, and the correct principles of delivering it through the different straits of the pelvis after the head has been decerebrated and crushed.

Analyzing these cases, and even from the highest sources, the unfavorable results will be traced to unnecessary and culpable delay, procrastination in resorting to artificial delivery, which was put off too long, or, if adopted, was continued till the patient was in a state of extreme exhaustion, and *death* the *result.*

The excessive deformities we are considering diminishing the pelvis fully if not more than one-half of its natural proportions, demand certainly one of the *two operations*, either the sacrificial act of the life of the child, "barbarous and horrible," as it has been termed, or the Cæsarean section as the *dernier ressort*, with the unfortunate sequences attending the operation, even if early performed, to the mother, whose chances, I believe, are far greater than by craniotomy and its addenda.

If embryotomy is to be performed, abandoning all hope for the infant, our whole solicitude should be directed to the safety of the mother. To a want of that necessary energy, decision, and promptness of action, at the opportune moment, is, as I conceive, to be attributed the unfavorable and unfortunate issue in many cases.

Charrière, in his excellent memoir, in relating his experience on this point, says that, "when *bad results* occur, they must be attributed chiefly to the causes which require the

accoucheur's interference, mainly in lingering and powerless labor, bad or incessant contractions, all of which produce such a state of extreme nervous exhaustion as to place the patient in the worst condition."

The statistics of Dr. Jones, of Paris, in 1847–'49, when cephalotripsy was performed, show that in pelves whose conjugate was not less than two and a half, out of eight women, five died. Dr. Greenhalgh in six cases; diameter not below two and a half, five died. According to Dr. Mundé's report of the Vienna School, forty-three cases of cephalotripsy; twenty-one deaths, fully one-half; in a conjugate of two inches to four inches. Henning, out of two hundred cases of cephalotripsy, thirty-nine died, or nineteen and a half. Dr. Parry, of Philadelphia, out of seventy cases collected, 61.43 per cent. recovered; fully one-third, 38.57 per cent., died. M. Joulin out of two hundred and fifty-three cases of cephalotripsy, representing, in mothers and children, five hundred and six existences; mortality sixty-five per cent.

On the other hand, thirty-seven cases have been collected, in which the employment of energetic manual and mechanical force was resorted to by his compression forceps. In these cases there were seventy-four existences, and the general mortality was not more than forty-three per cent. This result gives a difference of twelve and two-thirds in favor of energetic treatment and traction.

By the cranioclast (Rokitansky): In five cases, diameter of two to three inches, all the women recovered; child five and a half to six pounds. Dr. Parry, from the statistics of Harris, and Pahan Dufiellay, on the Cæsarean section, and his cases of craniotomy, considers there is nearly ten per cent. in favor of Cæsarean section. Out of the seventy cases I find

No time is fixed in	40	cases.
From one day to four or five days	21	"
One day	5	"
Uncertain	3	"
Total	69	"

The cases of Greenhalgh were treated by the perforator and

crotchet; Jones and the Vienna school, by the cephalotribe; and Rokitansky's by the cranioclast of the same school.

Before referring to the case of kyphotic pelvis, in a living subject, that of Mrs. V., I will present you with a cast for inspection of the Robert pelvis of Cologne. It is a transversely anchylosed, contracted pelvis, diminished in every diameter, especially the inferior strait. It might be taken for a girl's pelvis of the funnel-shaped order. I present this specimen on account of the inferior strait representing the outlet as nearly the same as that of Mrs. V. between the tuber ischia, although Mrs. V.'s pelvis was in the antero-posterior diameter from the pubes of the sacrum to the pelvis one inch less; coccyx bent up. It is an exceedingly rare specimen of deformity of the female pelvis, and there are no evidences of a constitutional character tending to a rickety or malacosteon pelvic deformity.

The measurements of the Robert pelvis of Cologne are—

Antero-posterior diameter, superior strait	$3\frac{7}{8}$
Transverse	$2\frac{7}{8}$
Transverse (in the cavity)	$2\frac{1}{8}$
Between tuber ischia	$1\frac{7}{8}$
" rami ischia	$1\frac{3}{8}$
" " pubes	1
From coccyx to pubes	$4\frac{1}{4}$

The measurement of Mrs. V.'s pelvis, which was of the kyphotic order, the promontory of sacrum more elevated, as is usual in these pelves, and not reached by the fingers, was presumed to be five inches. Left side of the pelvis jutting inward, and narrower than the right (*see* Fig. 3):

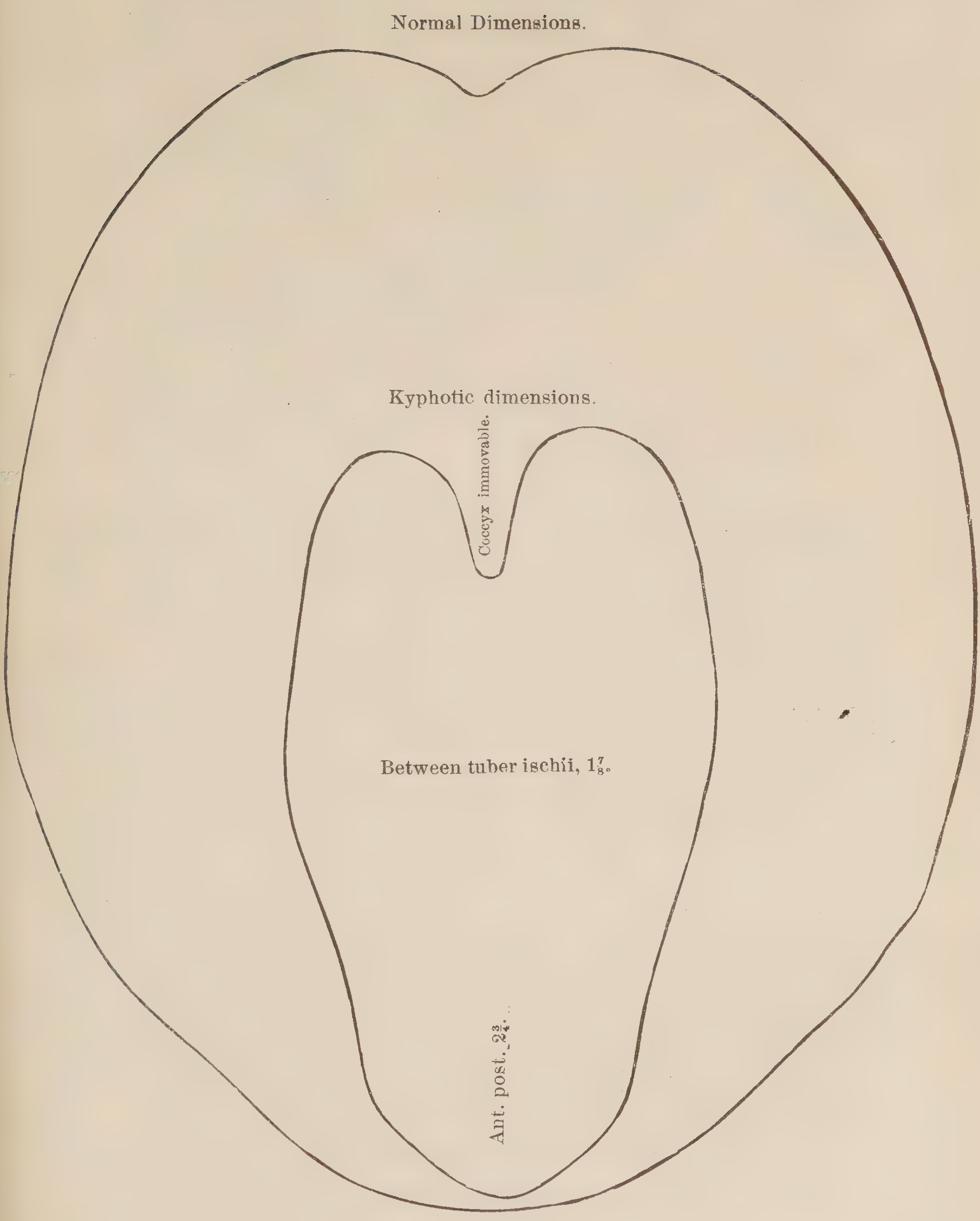
Transverse (in the cavity)	$3\frac{3}{4}$
Between tuber ischia	$1\frac{7}{8}$
" rami ischia	$1\frac{3}{8}$
" " pubes	$1\frac{1}{8}$
Coccyx to pubes	$2\frac{3}{4}$

The kyphotic pelvis of Lange—

Antero-posterior superior strait	$4\frac{1}{8}$
Inferior antero-posterior	$3\frac{7}{8}$
Between tuber ischia	$1\frac{3}{5}$

Half-inch smaller than Mrs. V's.

Fig. 4.



Kyphotic Pelvis (Outlet) of Mrs. V——.

The transversely contracted pelvis of Lloyd Roberts, of Manchester, England:

Antero-posterior diameter, superior strait	4
Antero-posterior (cavity)	$5\frac{1}{8}$
Transverse, superior strait	$3\frac{3}{4}$
Transverse (cavity)	$3\frac{3}{4}$
Antero-posterior inferior strait	$4\frac{3}{8}$
Between tuber ischia	$1\frac{1}{4}$
" rami ischia	1

Three-quarters of an inch less than the Robert pelvis of Cologne.

In Simpson's case of contracted pelvis:

Antero-posterior superior strait	$3\frac{3}{4}$
Transverse	$3\frac{3}{4}$
Antero-posterior inferior strait	3
Between tuber ischia	$\frac{1}{2}$
From coccyx to pubes	3

In this class of deformed pelves the contraction is at the inferior strait or outlet, while in the rickety or malacosteon pelvis the diminution is generally at the superior strait, with amplitude at the outlet. They are the reverse of each other. The inferior strait will represent very nearly the appearance of the superior strait of a malacosteon pelvis. Simpson remarks in his case, "that the spine projected to a considerable extent at the upper part of the back, and again to a smaller extent in the sacral region. The capacity of the brim, however, was not in proportion to the three and three-quarters inches antero-posterior, and three and three-quarters inches transverse, and collapses inside of the sides of the pelvis. The outlet was only half-inch transverse." From the description of Simpson I am induced to believe that the pelvis was one of the kyphotic order; for Simpson says, as confirming this view, "The osseous tissues appeared too firm for a rickety pelvis." In all these cases, except my own, the Cæsarean section was performed, and the results were unsuccessful—all dying.

The *kyphotic* pelvis is consequent upon some affection of the spine producing a kyphosis, either in the lumbar region or the lumbo-sacral region.

The causes, as a general rule, have been from falls and blows on the spine or pelvis. Out of eighteen cases, fourteen were from accidents of this kind. The absence of kyphosis and the difference in the transverse diameter of the false pelvis distinguish the double anchylosed pelvis from the kyphotic. If the kyphosis occurs in the lumbo-sacral region, there is a greater deformity in its form and shape transversely. The pelvis has no tendency to a rickety or malacosteon type, although in the Brussels pelvis, and the one by Mohr, there was some mobility of the joints observed, as we notice in some cases of *ramollissement* of the joints during gestation in natural-formed pelves. If there is lordosis with kyphosis the difficulty will be aggravated in the superior strait or just above the brim, and give the appearance of a spondylolisthetic pelvis, that is where the lumbar vertebræ slide over or down from the sacrum into the pelvis. This was the case in the pelvis described by Olshausen, which was diminished in the antero-posterior diameter to three and two-fifths inches, and that of Gluge, of Brussels, to three and two-eighths. The same with Fehling and Hatin.

There are records of only eighteen women with thirty-six children, and eight deaths of mothers. In the eighteen cases, premature delivery was adopted in eight cases, and four of these had to be delivered with forceps. There were full-born children four times. Forceps used in ten cases, in two of which they were of no avail; with these *two* cases and nine others, embryotomy was resorted to. The Cæsarean section was twice performed; one died, one recovered. Undelivered two, make fourteen. Out of the thirty-six children, twenty-three died, two-thirds during, or at least by, the delivery; thirteen children saved.

I have at present under observation three cases of slightly oblique ovate pelvis, consequent on coxalgia in two, with a contraction at the inferior strait as small as Mrs. Valentine's, and one of these patients was presented before the class January 13, 1876—the one on whom I operated in 1870 successfully.

The final result in all these cases will be the performance of craniotomy and cephalotripsy, as the head of the child can enter the brim of the pelvis antero-posteriorly, while in the transversely contracted or double anchylosed pelvis, of the type of which I have given you an illustration, the Cæsarean section will have to be performed. Out of seven cases of this nature, six operations were by the Cæsarean section.

I was requested by Dr. Sprague, March 4, 1875, to visit Mrs. Valentine at 8 P. M. Labor had commenced at 11 A. M.; pains every fifteen or twenty minutes, slight and inefficient. On examination the os uteri would scarcely admit the end of the finger, the edge being very thin. The head of the child was capped by the expanse of the cervix, and jutted into the pelvis. As the pains proved of little avail, she was ordered to take a full dose of opium or morphia, and to be seen in the morning.

March 5th, at 9 A. M., I met Dr. Sprague. Patient obtained some sleep, and the pains for the last three or four hours occurred every fifteen to twenty minutes; they were longer in duration and seemed to have more effect. The os uteri was opened to the size of a five-cent piece, seven-eighths inch in diameter. Next visit at 2 P. M. As some of the gentlemen I had invited were prevented from being present, Dr. Sprague secured several of the house-staff of the Bellevue Hospital, in case it should be deemed necessary to have assistance for the Cæsarean section, as it was contemplated that this operation would have to be resorted to, by some of the gentlemen who had seen the case previously. At 3 P. M. there was no change; os uteri the same; head well flexed and presenting with the occiput anteriorly. My slender and narrow curved forceps were applied easily, the object being to fix the head by means of them against the os uteri, retain it there during and after a pain, using but little if any traction, as the forceps could add no additional width to the head, and they were an efficient aid to Nature. Without an operation of this kind, the head would have remained some hours longer without any advance, as we had noticed before. The continuance

of the head in this position in the cervix would have produced the unfortunate results consequent on delay, by pressure on the internal organs and the pelvis. As the forceps were safely applied, the reason for their application was to allow the head to become the dilator with their assistance, as it does naturally. The cervix became expanded fully one-half, if not more, in twenty minutes; the thin-bladed forceps were removed, and the larger and stronger pair of instruments I am accustomed to use substituted. After gentle traction the os uteri was more dilated, and the head lower in the cavity of the pelvis; the cranium was then perforated, the cephalotribe applied, and firm pressure with the ratchet made, and the head crushed, till the handles were in apposition. As the head was now in the cavity of the pelvis, the base of the cranium could be reached more efficiently to break it if necessary. The cephalotribe had accomplished all that was expected from that instrument, and was removed. My right-angle blunt-hook (Fig. 8) was then inserted into the vault down to the base, and the head brought down to the inferior strait obliquely. Meigs's forceps, with an addition of four inches I had made to them (Fig. 7), were then resorted to. One blade was thrust into the cranium down to the base, and, the other grasping the head externally, the head was delivered sideways without much if any difficulty, and the body by steady traction was soon born.

The delivery did not require as much strength or force as, if any more than, some forceps operations in generally narrow pelves. While waiting for the placenta to be delivered, a few moments after its expulsion, a rapid and full stream of blood took place from the uterus, as if a Croton-water faucet had been turned. Instantly I used very strong and powerful flagellation over the uterine region, with as much force as could well be given to a towel doubled up, saturated with ice water, and as instantaneously was the hæmorrhage arrested. The round, globular uterus was now felt under the hand. An electric shock could not have acted more efficiently. (I much prefer that the ice should be finely crushed and placed in the middle of the napkin, and the two ends held for application.) By this

method, mildly severe, I have for many years succeeded in producing prompt and ready contraction of the relaxed uterus. After contraction has once occurred, the case is to be closely watched, and the same steps occasionally adopted if necessary, while other means may be resorted to. Internal injection, or application, no matter from what kind of remedy, would in this case have been a mere *bagatelle*, and perfectly useless. After the delivery the patient was removed to bed, she had a comfortable night, sleeping the whole evening, and was bright and cheerful in the morning. In twelve days she was walking round the room; time of delivery, one and a quarter hour; child eleven pounds.

A marked difference of opinion, within the last few years, has been manifest respecting the two operations. The want of success in craniotomy and cephalotripsy, and the unfavorable consequences attending the operation, as it is believed by some, even with the use of the cephalotribe, have had a tendency to turn aside the operation and lead them to resort to the Cæsarean section as preferable.

Modifications in the cephalotribe have been made, in accordance with the views of obstetricians, very different from the older one of Baudelocque, as the instrument of Dr. Kidd, of Dublin, will show. At the present day, the English have modified the instrument so much as to lead us to consider that it has attained almost a proper and perfect form. In France, the instruments of Pajot, De Paul, and Guyot, are considered excellent instruments. They are different from those of Braun, of Vienna, and Scanzoni, of Wurtzburg, which are heavier and larger. The instrument I have been accustomed to use is a modification of the French, but lighter, narrower, with a slighter curve, and more grooved in its blades. Since the improvements have been made in the cephalotribe, some obstetricians entertain the hope that from craniotomy and cephalotripsy it will be possible to extend the extreme limit of craniotomy to one inch, or one and a quarter inch.

Of the fifty-one cases referred to in Table 1, of Dr. W. H. Jones's work "On the Management of Labor in Contracted

Pelves," nine presented a contraction under two and a half inches; eight terminated by the mutilation of the child, and six mothers died; which gives a proportion of sixty-six per cent. The children in these cases only averaged four and one-quarter pounds. Dr. Jones remarks that, "if we consider that the total mortality of mother and child is fourteen out of eighteen, we are led to deplore the impotency of obstetrical art in cases of this description;" and that in these cases "it is the *nature* of the operation, rather than the *duration* of labor, which proves destructive; that when cephalotripsy is performed in a narrow pelvis, it is a murderous operation not only for the child, but for the mother."

Still, under this very unfavorable aspect of the operation, while alluding to a case of Cæsarean section of De Paul's, he remarks: "As he is unsettled in his mind respecting the two operations; that as there is another operation besides the Cæsarean section, that would give a chance of safety to the mother, the diminution of whose pelvis was not below one and three-quarters, I would not hesitate to practise it."

Greenhalgh, in his paper, in the Transactions of the Obstetrical Society of London, on the comparative merits of Cæsarean section and craniotomy in cases of extreme distortion of the pelvis, gives six cases of extreme distortion of the pelvis, in which the conjugate diameter did not exceed two and a half inches, requiring the use of the perforator and crotchet. All had reached the full term of pregnancy; all had suffered more or less for from thirty-two to seventy-two hours; time of extraction, three to thirty-two hours; five died in two to seven days, either from exhaustion or some inflammatory lesion; two uteri and two vaginæ were ruptured. The others recovered each after a severe attack of peritonitis; one underwent the Cæsarean section, and died the fourth day. From this exhibit, Greenhalgh thinks that craniotomy and crotchet cases are only safe within certain limits, and under certain conditions. Nothing would induce him again, under even the most favorable circumstances, to attempt delivery by the crotchet where the conjugate diameter of the brim does not fully measure two

inches, exclusive of the soft parts. I most fully coincide with Greenhalgh, especially when instruments of that kind are used, and with such delay attending the labor and operation.

Dr. Parry, in his paper on "Craniotomy and the Cæsarean Section in Small Pelves" (*American Journal of Obstetrics*, vol. v., No. 4, February, 1873), in his summary at the conclusion of his paper, says that, "if gestation has advanced to the full time, and the conjugate is two and a half inches, craniotomy affords the mother no better chance of recovery than the Cæsarean section;" and that, "if the diameter be two inches or less, exclusive of the soft parts, it is the duty of the accoucheur to perform gastro-hysterotomy, rather than craniotomy." I have cited these opinions particularly, as they are the most recent on the subject.

From the great mortality which occurred in Jones's cases, taken from the clinic of Dubois, who, as I have said, considers that it is the *nature* of the operation, rather than the duration of labor, which proves destructive, I think we need not be surprised at the opinion expressed, when we reflect on the method of the management of the cases of Dubois, as well as Greenhalgh and Braun. The average duration of the labors was forty-five hours. We are all conversant with the fact, for it is *an established one*, that the danger to the mother and child increases in a ratio proportionate to the duration of labor. Now, where all the unfavorable results follow the delay of an operation which should have been performed earlier, in conformity to this law, the *nature* of the *operation* could not be charged with the unfavorable issue, unless the operation has been bunglingly or badly performed.

Let us take a retrospect of two or three of the cases where the cephalotribe was used, as it is only by studying clinical cases that we may form a more correct and just opinion respecting the method of application of the instrument, and what its special action and purpose are, as some of these cases have influenced the minds of obstetricians in favor of Cæsarean section. A mere mention of the steps adopted would not convey a just appreciation of the method.

Case IX.—This case is one where the antero-posterior diameter was two and a quarter inches; eighteen hours after cephalotripsy, version was performed. Dubois saw the patient nine hours after labor.

August 16*th.*—7.30 A. M., os uteri as large as a crown-piece; 9 A. M., complete. M. Dubois ruptured the membranes at 4 P. M. Seven hours after, as the labor had made no progress, craniotomy was performed by M. Taurin, cephalotribe applied, and the head crushed; but this could not be brought down by the strongest traction. Patient left till 5.30 P. M. Cephalotribe again used, but without being able to get a good hold on the head. M. Dubois again tried, but had great difficulty in inserting the branches of the instrument, on account of the anfractuosities caused by the broken bones of the head. After two more efforts, adjourned further operation till 8 P. M. At 8 P. M. everything *in statu quo.* Pills of extract of opium given, and patient left till 7 A. M., an interval of eleven hours.

17*th.*—7 A. M., patient the same, as regards the labor; skin hot and dry; pulse 120. At 9 A. M. M. Dubois (after twelve hours' interval) made another attempt to employ the cephalotribe, but in vain. M. Taurin was requested to turn (twenty-four hours after first visit). The foot was seized and with great difficulty brought down, but the other leg could not be. Cephalotribe applied to the pelvis of the child, but unsuccessfully. Patient under chloroform for half an hour. Patient pale, and facies looking badly; all operations were suspended. At 10.30 severe rigors. At noon (12 M.) M. Dubois came, and advised further traction by the foot. Traction was made for an hour and a half, till 1.30 P. M., and then, with the crotchet, the patient was delivered. The mother expired directly afterward. The weight of the child was five and a half pounds, with the cerebrum. From the time the membranes were ruptured, when the os uteri was fully dilated, eighteen and a half hours had elapsed before the delivery of the child. Autopsy gave: vagina torn; uterus not completely ruptured, but having a rupture of a finger's shape penetrating almost through its tissue. The dimensions of this

lesion corresponded in size with the blades of the cephalotribe.

M. Pajot adopts a method which he calls "*cephalotripsy* without traction." Satisfied as M. Pajot is of the difficulties attending cases of this nature, he proposes that, "as soon as the os uteri is sufficiently dilated, the cranium of the child be perforated, before the dilatation is complete, with the view of facilitating it, as the process of dilatation is often slow in cases of extreme narrowing." Of the *seven* cases *operated* on by Pajot, as witnessed by Jones, five were successful and two unsuccessful. In one of these cases Pajot admits it was impossible to break the bimalar diameter, which proved the only obstacle to the passage of the head. The *method* of Pajot has been considered as a kind of lithotripsy of the head, and which occupies fully as much time as, if not more hours for delivery of the child than, M. Dubois's. Can this method of Pajot, taking into consideration the length of time employed in effecting the completion of the labor, even if the head was crushed, have any advantage over that of M. Dubois by strong traction? Are not both methods to be looked upon as offering no more favorable results for the mother than does the procrastination or delay theory and treatment taught by Denman, Collins, Lee, Murphy, and others, many years since, and still adopted by some at the present day? Pajot and Dubois make the attempt to deliver the woman by the cephalotribe, but the long time it occupies to effect the delivery is *nothing more* than *testifying against* the *law* which is recognized as true, that "every hour after the rupture of the membranes entails great danger to the mother and *child*."

In principle and result I can see no difference from the opinion, as expressed by Collins, that, "when it unfortunately happens, as in some instances which are unavoidable, in consequence of the protracted length to which we are at times compelled to permit the labor to proceed, owing to great difficulties in the passage of the head, the *child* being *alive*, the medical *attendant's mind* cannot, on his own account, feel distressed, as the only means he could adopt to guard against the

danger would be to lessen the head of the child, which, *in my* opinion, no *consideration should induce* him to do under such circumstances." With the method of such treatment the accoucheur waits quietly and patiently for the spontaneous death of the child before he has *the right* to interfere. Delay from such treatment plays the *rôle* of the perforator and the cephalotribe. But what of the mother? Let me now refer to a case which is recorded by Dr. Mundé, in his paper on "The Cranioclast as improved and used by the Vienna School" (*American Journal of Obstetrics*, May, 1873); reported also as Case LIII. in Dr. Parry's paper, same journal, February, 1873. The case was published by Dr. O. Franque, and referred to by Dr. Mundé:

D. P., aged twenty years, was admitted into the hospital with labor-pains, February 21, 1868. Her extremities were considerably deformed by rachitis, and the pelvis found generally contracted, the external conjugate being three inches and two lines, the internal two inches and five lines; left half smaller than the right side. Child dead. Perforation was performed, and Scanzoni's cephalotribe applied. This instrument, after having slipped and been reapplied eight times in different diameters, by different operators, was discarded, and podalic version attempted. As the contraction was so great, it was found impossible to pass the *arm up* far enough (contraction was only two inches and five lines) to reach and grasp the feet or knees of the child, and after five *distinct attempts* had been made in various positions of the mother, and by three different gentlemen, we were compelled to resort to other means, and seriously thought of the unfortunate *dernier ressort* of Cæsarean section. Before proceeding to this extremity, we concluded to try the various bone forceps, and with Simpson's cranioclast and the crotchet we succeeded in removing a large part of the skull, and aided by traction, as one of the arms had been drawn down, in gradually pulling at the basis cranii into the superior strait. It was now possible to seize and crush the head with the hand, and I finally made the manual extraction of the child some ten hours after the per-

foration. Child weighed, with contents of the crania, six and a half pounds; without, five and a quarter pounds. Mother died on the fourth day. "It was this case which induced Dr. Mundé to express his opinion that he had lost a great part of his faith in the cephalotribe." Dr. Parry, in his article published in the *American Journal of Obstetrics*, vol. v., February, 1873, presents the case of a colored woman, a dwarf fifty inches high, rickety, with antero-posterior diameter of an inch and a half, as he supposed, but which patient was attended afterward by Dr. Parrish, when it was recognized after the death of the patient, one month afterward, that the pelvis was an inch and seven-eighths antero-posterior.

March 14, 1872.—At 8 P. M., induced premature labor; at 9 P. M. os uteri dilated rapidly; craniotomy performed. Perforation difficult through the right parietal bone; brain evacuated and crotchet applied; no success. Hicks's cephalotribe tried; no success in the operation. Simpson's cranioclast tried; no success. Meigs's embryotomy-forceps used to pick away the skull, continued one hour. The same with the cranioclast; crotchet tried again. 11 P. M., patient very low; cephalotribe again tried to be applied. One blade supposed by one of the gentlemen to have passed through the uterus and the peritoneal cavity; no success. Delivery could not be effected by either crotchet or cranioclast. Meigs's forceps again resorted to for one hour. After trying the crotchet again, at 1.30 A. M., and with a strong effort, the head was found to engage in the superior strait.

"We then," Dr. Parry says (four and a half hours), "succeeded in breaking away a few remaining portions of the frontal bone, when we brought down the chin in the axis of the superior strait, and the head was seized by the cranioclast (that is, the face), when the child was dragged into the world, to the great relief of all present. Time of delivery, six hours." In this case of Dr. Parry's the head of the child was not made a face-presentation till fully five and a half hours elapsed, and it was by this procedure after the decerebration that the delivery was accomplished, but not till then. In March, 1874, two years later,

Dr. Parrish delivered this patient, after destroying the cranium and making it a face-presentation, and then by Meigs's forceps breaking up the base, but not till this was accomplished was the child delivered. Time, two hours. Patient died one month afterward. Antero-posterior diameter, an inch and seven-eighths.

This single case of Parry's induced him to draw the conclusion in favor of the Cæsarean section. I cite the cases of Parry and Parrish as illustrating the management of the delivery, by making, after the vault of the cranium was broken up, a face-presentation; but this was just previous to the complete delivery of the case, and that was through the action of the cranioclast crushing the face of the child. In Dr. Parrish's case the child could not be, and was not, delivered until the crushing of the base of the cranium had been accomplished. This method of treatment (so different from that of Dr. Braun or Pajot) was an appreciation of the relative proportions which exist between the diameter of the child's head and the base, and the diameter of the mother's pelvis in that kind of case. The bimastoid or the byzygomatic diameter of the child's head in each of the cases cited could not have been less than two and three-quarters inches or three inches, and unless that portion of the child's head had been fractured, or it had been made a face-presentation after complete destruction of the vault of the head, and thus brought edgewise, or it may be sideways, even without breaking the base, it could not have been delivered.

In none of the cases of Dubois, or Scanzoni, or Braun, was the face of the child made to present edgewise independent of the crushing the base or bringing it sideways, so that the smallest diameter of the child's head could present, that is, one half antero-posteriorly the dimensions of the child's face and the base transversely placed. It must be apparent, therefore, that in neither of these cases was it to be expected that the cephalotribe as a means of delivery solely, which they relied on, could succeed, as the head was movable above the brim, and the instrument could not reach high enough to produce any fracture of the base, and, if not accomplished, it could not act as a tractor to deliver the child, and, without

this object being obtained in these cases, the cephalotribe becomes a useless instrument.

The frequent repetition of crushing the head according to the method of Dubois and Pajot must entail delay, incident to it, which would produce increased exhaustion, and all the unfavorable consequences attending it, which I need not reiterate.

Dr. Hicks, in 1864, called the attention of the profession afresh to the views of Drs. Hull and Burns, who state that, "if the whole calvarium of a full-term fœtus be removed so that only the base of the skull be left, it will be readily perceived that the relations of the diameter are altogether altered. The head is then to be made a face-presentation, with the chin directed to the pubes, and the hollow of the skull to the promontory of the sacrum." Dr. Hull, the bitter opponent of Dr. Osborn, remarks as the result of his experiments, that "the head so diminished can be applied to a small aperture with a view of bringing the face edgewise, but not with the occiput foremost (for in this case the volume of the neck must be added to that of the face), not sideways, as Osborn states he placed it in the case of Elizabeth Sherwood." Here is the essential point of so much value and importance to a just comprehension of the difficulty in the extreme narrow pelves, and upon which has hung the lives of many females, who might have been saved if this principle of treatment had been properly appreciated.

The views of Burns are clearly enunciated, for it was Burns who culled from the heated and bitter discussion between Osborn and Hull and others a principle of treatment, which is, and will be in some cases, of so much importance in the delivery of the patient safely, and which has been so much neglected and overlooked, though it differed entirely from Osborn's. Hull and Burns endeavored to combat the opinions and experience of Osborn, although they were derived entirely from one case only of extreme narrow pelvis, the pelvis of Elizabeth Sherwood, which measured in the antero-posterior, direct from the promontory of the sacrum to the

pubes, only three-quarters of an inch. On the right side of the pelvis the antero-posterior was one and seven-eighths and two and a half transversely; in fact, the same measurements as Mrs. V.'s, hers being in the inferior strait. They hold that after excerebration and destroying the calvarium, and then making the head a face-presentation, occiput posterior, chin in front, the head would come edgewise, face first, while Osborn's opinion was that in his case the head came sideways, and not face edgewise. They consider this view of Osborn's as wrong, and that if it was, it was, as Bland asserts, only by accident. This is a great injustice to Osborn, for he expressly states that, "after having made very powerful efforts at traction with the crotchet and to no purpose, for I could not perceive that I had made any impression on that solid bone, the base, or that it had been the least advanced by all my exertions, I therefore abandoned altogether the idea of breaking up the base of the cranium, and determined to try the second by endeavoring to change the position. I therefore again introduced the crotchet in the same manner, and, fixing it in the great foramen, got possession of my former purchase; then introducing two fingers of the left hand, I endeavored with them to raise one side of the fore-front of the head, and turn it a little sideways or edgeways. Immediately and easily succeeding in the attempt, the two great objects were at once accomplished, for the position was changed and the volume diminished. Continuing my exertions, the head advanced and I found a considerable portion of it brought into the pelvis, and shortly delivered." This procedure was certainly not an accidental circumstance in the delivery of Elizabeth Sherwood, but wisely conceived and promptly executed, not by making a face-presentation, but by tilting or slanting the base obliquely, which I have noted frequently in a naturally-formed pelvis, as Nature asserts her prerogative in the delivery of the child, whether synclitically or obliquely, and from her instructions we are to profit, and would profit if they were carefully studied. Osborn may have been too hasty in asserting that he could deliver with the crotchet where the diameter, an-

tero-posteriorly, was one and a quarter inch, but this does not detract from his well-earned success in the case of Elizabeth Sherwood. Sarcastic and violent as the controversy was, valuable information has been deduced from it, for we learn that it is possible for a child to be so mutilated and treated as to be delivered through a contracted brim, or inferior strait, where the diameter may be as small, or narrow, as one and a half inch at least.

From the case of Osborn and the discussion which followed, and from the experience obtained in my own practice, it is clear to my mind that there are certain principles which should guide the obstetrician in the treatment of those unfortunate cases with which we may meet, and, by comprehending the adaptation of the smallest diameter of the child's head to the smallest diameter of the mother's pelvis, the delivery would now be considerably advanced in obedience to the mechanical law Nature has established, that the smallest diameter of the head of the child should be opposed to the smallest diameter of the brim, and the largest to the largest. Without the comprehension and appreciation of this obstetrical fact, respecting the method of delivery, all the instruments that might or could be used would only tend to subvert and set aside an operation that should rest upon the aid of only one or two instruments to effect the delivery, and which should be accomplished without great prejudice to the soft structures of the mother.

On the Continent the cephalotribe has been used for many years. In Great Britain, although recognized, it has only within the last few years, from eight to ten, claimed the attention which is now given to it. As late as 1868, Hicks, who was a strong advocate for destroying the head of the child by the crotchet in 1864, has, since 1868, employed it after perforation. He says: "I have not had occasion to use the crotchet or craniotomy-forceps, and am confident it has shortened the delivery by three-quarters of the time that other instruments would take." The cephalotribe is a compressor of the vault of the cranium, and seldom more so. It has been

conceded and demonstrated, that in many cases the base is not fractured or crushed, but that the vault is bent or turned upon itself.

This evidence comes from the testimony of the authors themselves, in the use of the instrument. It is admitted, however, that, on account of the mobility of the head, it is very difficult to obtain a firm hold, and hence the frequent slipping off, or away, of the instrument. Application after application has, however, been resorted to and failed, and as many as ten or twelve times it has been attempted without much success. It has been supposed and believed that, when it was well and properly placed, the screw applied, and the handles brought together, it would retain its hold; but it sometimes changes its position, and the bimastoid is thus avoided and the base not broken—one of the marked and essential *features* claimed as the object to be gained in the use of the instrument originally.

Another important objection to the instrument is that, after the withdrawal of it, if the base has not been crushed, the vault will expand, or resiliate, sometimes to the extent of fully one-half or three-quarters of an inch, producing those irregularities of the skull which place a barrier against its being easily and correctly adjusted again. The cases of Dubois, Parry, and others, attest this, and my own experience confirms it. On the cadaver, it is admitted that it is very difficult sometimes to effect the fracture of the base.

In the application, if the base is reached, it will sometimes require considerable force and strength in the instrument, which it may not possess, and a different instrument is therefore needed for that purpose.

Traction with the instrument, if well placed, may occasionally succeed in drawing down the child, if it is only five or six pounds, as in some of the cases cited, and effect the delivery easily (for I exempt pelves of three to three and a half inches); but, in the pelvis of the order under consideration, other instruments must be resorted to, more available to accomplish the termination of the labor. As a tractor in these

cases it is, I believe, of little value; in truth, it is almost useless, and valuable time is lost in making the effort. Another point is to be recognized. The exact position is not always known it takes upon the child's head, as the head is so obliquely placed and high up. We are not to be unmindful also, with these various objections, that there can be but one or at most two positions of the pelvis in which we can apply the instrument. It cannot certainly be applied antero-posteriorly, and therefore the space is limited. It must be introduced either in the transverse-direct or the obliquely-transverse. If the waters have been evacuated for some time—which is generally the case—and the head perforated, the head will be retained in but one position, and therefore cephalotripsy, according to Pajot's method, I conceive cannot be accomplished in the various diameters of the head of the child. As to the instrument rotating, when applied, the antero-posterior contraction confines the space of the transverse diameter, and narrows the field for operation to a space of two and a quarter by two and a half inches. This mathematical demonstration, with the remembrance of the width of the instrument being from one and a half to one and three-quarters of an inch, tends to set aside the views of those who believe in the frequency of applying the instrument, and crushing the cranium of the child in different parts of it. I believe it is a great error to suppose that the head of the child is crushed in its various diameters. If the remarks I have made are correct, then the frequent and oft-repeated attempts to use the instrument, and the long continuance of the labor, consequent upon the method adopted, are prejudicial to the welfare of the mother—incorrect and uncalled for. As a compressor of the vault, it is efficient; and it is invaluable in this respect, accomplishing its object quickly and perfectly.

The *cranioclast*, as an obstetrical instrument, has lately assumed an importance as an adjunct to the cephalotribe. It is, however, considered by some as being of more value, and that it might be substituted for it.

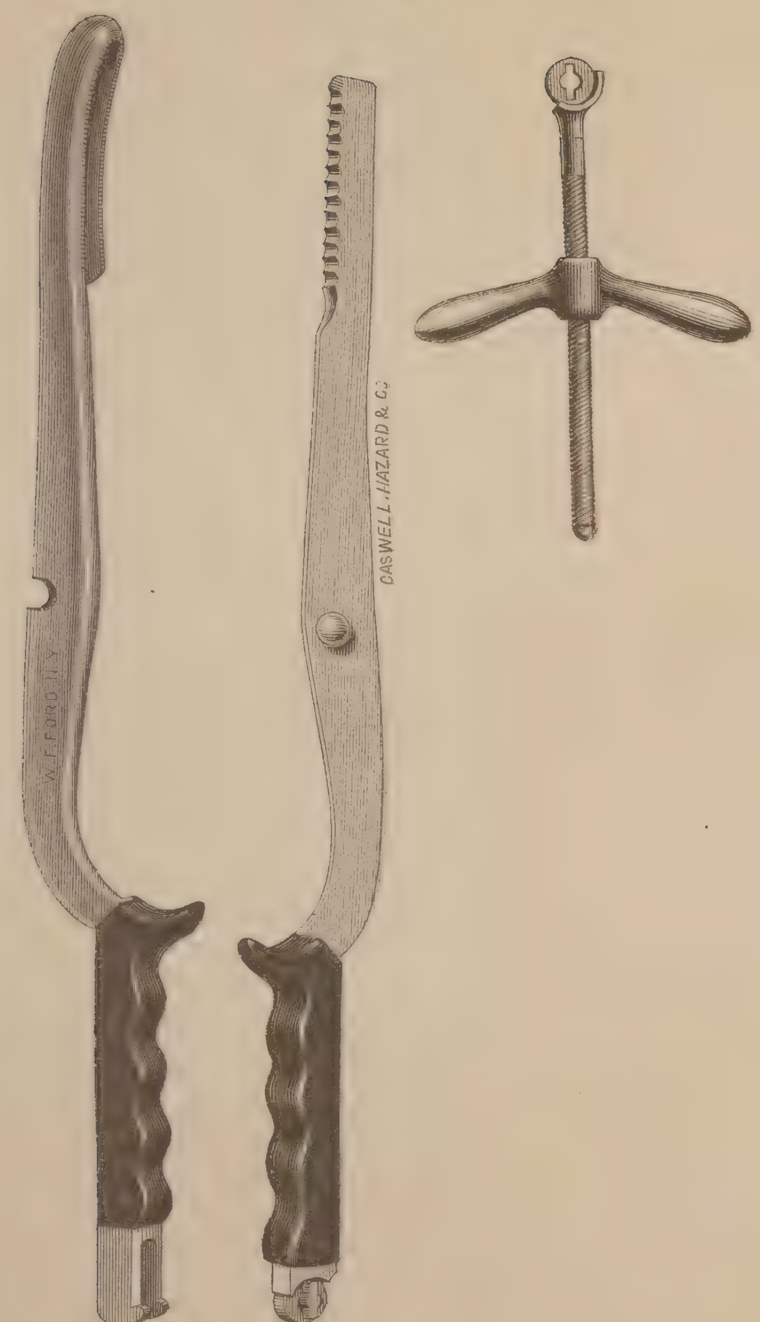
This instrument will also play only a certain *rôle* in the

management of the case, and it has, as I conceive, but a limited line of action. It is, as its name imports, simply a crusher—a breaker—and applicable especially to the solid bones, as the basis cranii, or on the face if the head is made a face-presentation after decebration. As a tractor, unless a very firm hold or purchase is effected, it has not sufficient power in pelves as small as one and three-quarters to two inches to drag down the child, unless the base is fractured and the obliquity given to the head. The hold that it obtains will frequently, by firm pressure and traction, twist or break the more slender flat bones, and more so if the child has been dead some hours and become softened, and then only a purchase of the scalp remains to pull upon, which is useless. If the child is large, the shoulders cannot be dragged through, and it fails utterly.

The cranioclasts of Simpson and the Vienna school are only a modified craniotomy-forceps. Simpson's instrument is almost identical with the craniotomy-forceps of Lee, a specimen which I present for observation. It is not powerful enough to fracture the base, if it is a strong healthy child, with the head well ossified; nor is it sufficiently long.

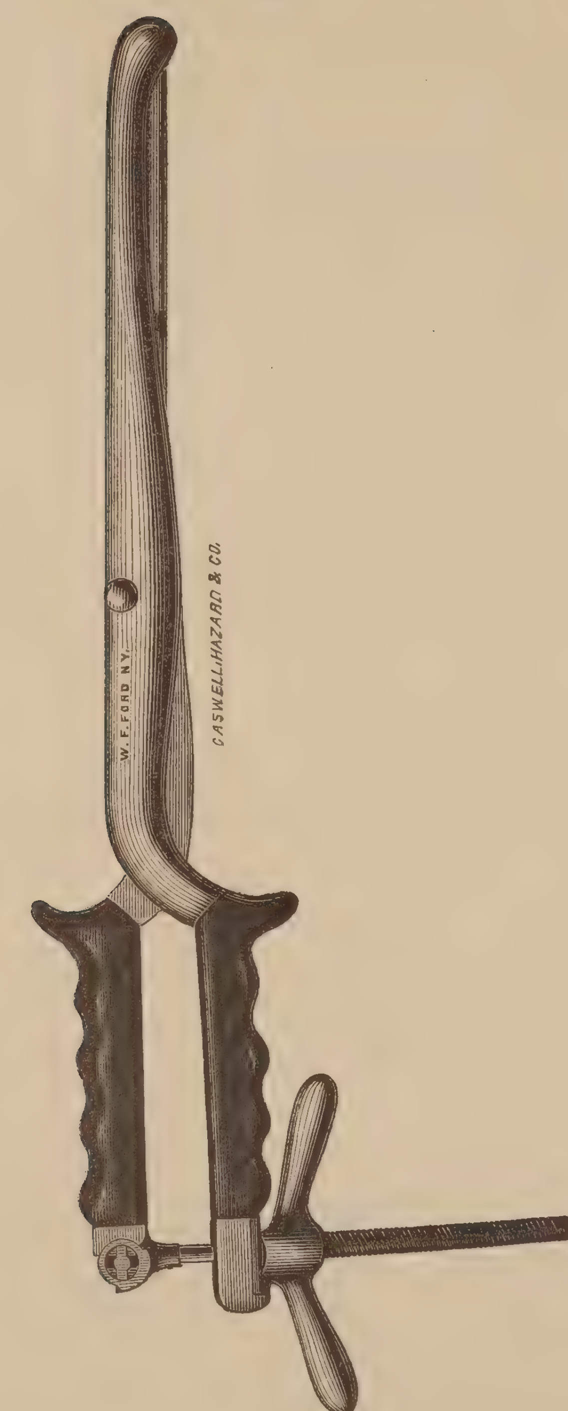
The cranioclast of the Vienna school is three inches longer, 1.4 inch in the handles, and 1.10 inch in the blades, than Simpson's—a much heavier and stronger instrument, weighing two and a half pounds—and has more curved and somewhat wider blades, and having a ratchet or screw attached. I do not think, from my experience, that this curve of the blades is any advantage but rather an objection when they are to be introduced into the cranium down to the base, as they cannot be easily thrust down to the base, or into the foramen magnum, if necessary. The curved blade cannot very well suit the convexity of the head where the convexity has disappeared by the collapse of the vault after the cerebrum has been evacuated. The *tendency* at the present day with some of the English obstetricians is to lean to instruments of a small curve, as we notice in the cephalotribe of Kidd, which has straight blades, and very long, with reversed lock and very short handles and ratchet.

Fig. 5.



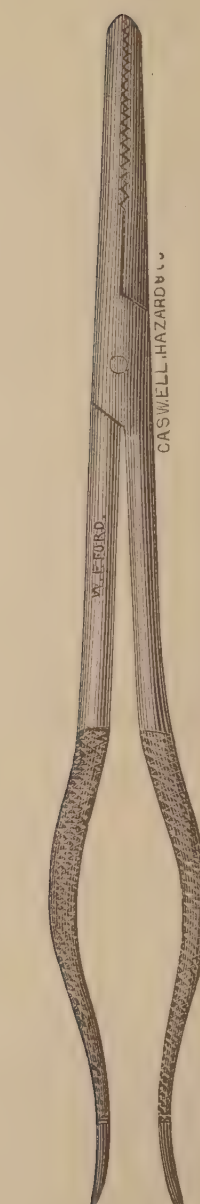
Dr. Taylor's Cranioclast: length, 15 inches and 3 lines; handle to lock, 9 inches; lock to lip, 6 inches and 3 lines; fenestrated blade, 3 inches and 1 line; solid, 2 inches and 6 lines; breadth of blades, 9 lines; weight, $1\frac{1}{4}$ pound.

Fig. 6.



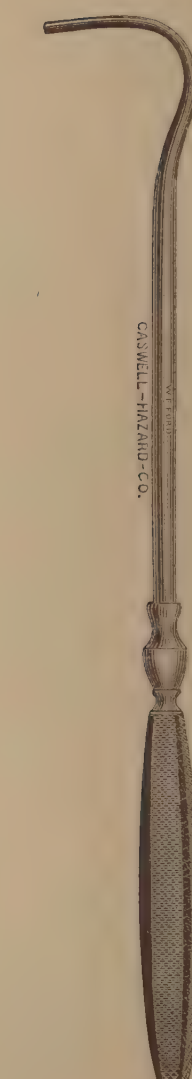
Dr. Taylor's Cranioclast: closed up.

Fig. 7.



Dr. Taylor's Modified Meigs's Embryotomy-Forceps: length, $14\frac{1}{2}$ inches; handle to point on lock, 10 inches; the point on lock to end, $4\frac{1}{2}$; breadth of blades at lock, $\frac{3}{4}$ inch.

Fig. 8.



Dr. Taylor's Right-Angle Blunt Hook: the tip of handle to end of hook, $12\frac{1}{3}$ inches; length of curve of hook, $1\frac{1}{2}$ inch; one more blunt than the other.

I offer for your inspection this evening the cranioclast I have devised. (*See* Figs. 5 and 6.) In doing so, I wish it to be clearly understood that I consider and use the instrument as a true crusher of the solid bones of the basis cranii, or the face, where required, although it may be used as a tractor, the same as the other cranioclasts. It is lighter, smaller in the blades, and only ten lines shorter in the blades than the Vienna instrument. It can be rotated easily, from the smallness of the blade, in the cranium at the base, or in the base; weighs only one and a half pound, and will crack or crush any solid bone. But, with all the benefits derived from these instruments, of whatever kind, after the base is crushed, if necessary, other instruments will have to come in to aid in the termination of the delivery. It is conceded even by those who advocate the cranioclast as a tractor, after the destruction of the calvarium and face by the instrument, that they are frequently foiled in their efforts even in pelves ranging from two and three-quarters to three and a half inches. They realize that it becomes therefore absolutely necessary, and imperative, to substitute other instruments for that purpose, despite their predilections and prejudice.

The instruments which have to be used are either the *crotchet* or the *blunt-hook.* I have always had an aversion to the crotchet, no matter whose; and I have used it very seldom, except where I could have no other instrument. I give the preference to the right-angle blunt-hook, and do not use the ordinary semicircular or triangle one. I have tried this hook (having two sizes) for over thirty years. (*See* Fig. 8.) I consider it decidedly more available than the ordinary hook, whether with a hard and solid stem, or one that is ductile. It can be introduced with perfect ease in any part of the cranium which is perforated ever so small, and traction made; or it can be passed, before perforation or after, on the under part of the chin of the child to effect a face-presentation without cutting the flesh as the other instruments do; or it can be thrust into the orbits, or on the sides of the head, and traction made without difficulty and considerable force, if necessary; or, to

effect version, if the breech does not evolve, it can be inserted easily into the anus and the child turned. The ordinary blunt-hook will not accomplish so easily the objects as the right-angled one.

As an expression of my own experience in cases of the diameter of one and three-quarters to two and a half inches, I do not believe that either the cephalotribe or the cranioclast will meet the requirements of the case without the aid of other means generally. Nor do I believe that the cephalotribe will be superseded by the cranioclast. Each instrument has its own especial province and field of action, distinct from those of the other.

As a forceps, *Meigs's* are invaluable, either the straight or the duck-bill, according to circumstances. They are too short, however, when the operation is at the superior strait. I have had them made longer by four inches, and somewhat stronger in the blades and handle, two inches longer in the handle and two inches in the blades. The blades are serrated more deeply. They are light and easily handled. The blades are united as an ordinary forceps, and are sufficiently long to reach the base. As tractors, with the additional length given to them, they are as available as, if not more so than, some other craniotomy-forceps I have used. (*See* Fig. 7.)

The remarks I have made on the subject, thus far, have reference to the management of labor in that class of cases after perforation of the head of the child at the superior strait, and the use of the cephalotribe or cranioclast, which is the established and usual method, or programme, for conducting the labor and delivering the patient, in preference to the Cæsarean section.

I do not confine myself to this method, believing there are others more efficient and practicable which may be resorted to. *Four* different courses can be adopted or carried out. They are:

1. Craniotomy, cephalotripsy, cranioclasm, and traction by the cephalotribe or cranioclast, the *ordinary method.*

2. Craniotomy, complete cephalotripsy, with destruction

of the vault, making the head a face-presentation, and to be delivered edgeways.—*Burns and Hull.*

3. Craniotomy, partial or complete cephalotripsy, delivering the head sideways by craniotomy-forceps, or crotchet or blunt-hook.—*Osborn.*

4. Craniotomy, cephalotripsy, with or without cranioclasm, and version performed directly after, with the trial of the blunt-hook, or crotchet, or forceps, whichever may be elected.

Regarding the first method, I have expressed my opinion. The second I have not found it necessary, though it may be in some cases. I much prefer the fourth with the third methods in pelves ranging from one and three-quarters to two and a half inches. The views which I presented in my first paper as to the management in simple *flat pelves* by *version*, in pelves ranging from two and three-quarters to three and a quarter inches, with propulsion from above the pubes, so clearly enunciated by Pugh, of Chelmsford, England, which was accepted by La Chapelle—which fell almost into obscurity—and which was resuscitated by Simpson in 1847, and lately advocated by my friend Dr. Goodell, of Philadelphia. Dr. Simpson advocated version in pelves ranging from two and a half to three and a half or three and three quarters inches to avoid the use of the long forceps, and to avert craniotomy if possible. Dr. Simpson, however, never refers to the important aid of pressure from above the pubes which Pugh has so distinctly advocated. Nor does he refer to pelves narrowed to one and three-quarters to two and a half inches, or even to Pugh's views as to the version. The last method I have carried out for twelve or fifteen years, in those extreme cases, after perforation and the crushing of the vault with cranioclasm or without. I am aware that after perforation, and more especially after the use of the cephalotribe has crushed the vault, version has been objected to, on account of the fracture of the skull by the perforator, that spiculæ of bone may cut or bruise the structure of the uterus. This, I believe, is a mistake. As a general rule there are seldom very rough edges or spiculæ, after a recent perforation, and if there were, they could be safely

covered by the scalp, which would be close over the opening made.

In the case of Dubois, and of Von Franque related by Mundé, turning was performed at the close of the labor, when the patient was exhausted, the uterine forces nearly expended, and as almost a *dernier ressort*, as the efforts before failed entirely, and this is the usual course adopted. May I ask, if the cranium of a child can be delivered without being perforated, having the diameter of the full capacity of a child's head at term, by *version* with *propulsion* from above the pubes, with traction, according to the amount of force necessary, and the proper direction through the axis of the pubes of two and half inches, as on the cranium I have presented, and as the history of cases from the highest authorities has certified to, why may not a child whose head has been craniotomized and decebrated by the cephalotribe so as to present only a diameter of one and three-quarters inch from the base of the head to the top, instead of three and a half inches, be treated by the same method, as in pelves ranging from three to three and a half inches, successfully? My own experience tells me that it may be done, and I have in some instances demonstrated this fact, before large classes, in pelves from two and a half to three inches, promptly and successfully. One of the cases of two inches, in a dwarf, I will shortly refer to, and as a contrast to the one of Mrs. V. I know that some authorities have fixed the limits of version at two and three-quarters inches. Schroeder informs us he has delivered living children within that space. I deem it far preferable to the course which is pursued by applying again and again the cephalotribe or the cranioclast, and making these instruments the lever or tractor of the child, with but little purchase, and is it not better to use the body of the infant as a tractor with propulsion from above?

Schroeder considers that, in narrow pelves of two inches and five lines, there is no hope *per vias naturales*. "In practice," he says, "as long as there is another way of delivery than Cæsarean section, which is very rarely permitted,

and in the highest degree of contraction, it will as a rule be necessary to perform craniotomy whether the child be alive or dead." He makes no reference to version early performed after craniotomy or cephalotripsy. Pinard, in his "Thèse de Aggrégation," lately published, is an advocate for version of seven centimetres, that is, two and a quarter inches. In pelves narrowed from five to six and a half centimetres, that is, two to two and half inches, he says that, if it be ascertained that the child is dead, version is then the only operation; but, if the child is living, version should be rejected and another operation substituted. Pelvic version is impracticable in pelves of one and seven-eighths inch he says. Nor does he say anything about version after perforation. Exceptions have to be made to version not only in those cases where the uterus is in a state of persistent or tonic action, or too much exhausted, but where the long axis of the child's body corresponds to the long axis of the uterus; then the delivery must proceed as begun, and the labor be terminated as promptly and safely as it can well be.

As an illustration of the views I have adopted in other cases of severe contraction of the superior strait, I will cite the following:

A. S., an Italian dwarf, forty-eight inches high, twenty years of age. No constitutional disease appertaining to rickets or malacosteon. Admitted into Bellevue Hospital April, 1865. Eight months advanced in gestation. The patient was placed in my charge by the Commissioners of Public Charities and Correction. There was a lordosis of the lumbar vertebræ. The opinion of my colleagues, Drs. Elliot and Barker, was, that the antero-posterior pelvic diameter was one and three-fourths inch; my own, that it might be two inches. Transverse diameter of the usual measurement, four and three-fourths to five inches. The Cæsarean section was considered requisite.

May 20th.—I was requested to visit her at 8 A. M. Pains every fifteen minutes. Patient looking and feeling well, bright, and cheerful, and naturally so. On examination the

os uteri was nearly one-half dilated; waters evacuated for half an hour. On external and internal examination I recognized the fœtal head occupying the left side of the pelvis obliquely. Preparations were made for delivery by the Cæsarean section.

At 10 A. M.—Present, my colleagues, Profs. Elliot, Barker, Sayre, Thomas, the house-staff, and other gentlemen. The patient was anæsthetized, and before proceeding to the operation I made an examination to decide more clearly the position of the child, when I perceived the head had changed its position, and a foot or hand was felt. On careful touching, I recognized it was a foot. This was seized with two fingers, and I decided to turn; and if not succeeding by that method, to resort to the Cæsarean section afterward. The foot having been seized, and external pressure made on the breech, the child was drawn down, and in a few moments the breech was brought through the superior strait, and delivered, finally the arms, and only the head remained. As I have done before in some cases, I brought the back of the child to the back of the mother, occiput posteriorly, face looking upward, so that the occiput should be placed in the left sacro-iliac space. By pressure from above by my house-physician, and retaining the head and gentle propulsion, with the child's body elevated toward the pubes, the occiput was reached, and the cranium perforated by Naegele's perforator. The brain was evacuated and steady pressure continued, and the head then pushed more over to the right sacro-iliac space, so as to be more transverse, and the smaller part of the vault, which, after the evacuation of the brain, would be only one and a half or one and three-fourths inch from the base to the top, instead of three to three and one-fourth inches naturally, brought against the promontory of the sacrum. The right-angle blunt-hook was then introduced into the lower orbit of the face (the right one), firm but gentle and steady traction made downward and forward, while the propulsion from above the pubes by Dr. Elliot was made backward and downward. After a few minutes' trial the cranium was forced

through the obstacle, and the child delivered. Child weighed nine pounds. Time of delivery, three-fourths to one hour. My little patient had a most excellent recovery without any untoward symptoms, and was walking about the room on the twelfth day.

I could add other cases where the antero-posterior diameter was from two and a half to three inches, and which were brought before the students after a few hours' labor. In some instances after perforation, the cephalotribe was used, and version at once adopted by two fingers internally, and external manipulation on the breech.

In pelvic presentations in this kind of pelvis I have pursued the same treatment of the body, the occiput carried posteriorly, as I have done sometimes in pelves ranging from three to three and one-fourth inches. I believe, for various reasons, it has an advantage over the opposite one, with the face of the child posterior, and back looking upward. On this point I will not now enlarge. This method is essentially necessary as an illustration in the obliquely-ovate pelvis, whether naturally so or created by diseases of the hip-joint or injuries. In two of the patients I have referred to I have delivered with pelvis in the inferior strait of one and seven-eighths to two inches diameter. They have since had abortion produced at the third and fourth month, and would not permit gestation to proceed to even six months. The other patient has consented to allow pregnancy to go to six or six and a half months.

From the observations I have made I deduce the following:

1. That a mutilated fœtus can be delivered with safety to the mother through a space of one and three-fourths inch antero-posterior, and two and a half or three inches transverse, by craniotomy, cephalotripsy, or cranioclasm, provided the vault has been destroyed, and the face made to present edgewise, or delivering the head sidewise.

2. That after cephalotripsy, or cranioclasm, if necessary, version, early performed, with propulsion from above the pubes afterward, and before the uterine forces are exhausted,

is preferable to the first proposition, and I believe more available.

3. That the cephalotribe or cranioclast cannot be considered sufficiently available as tractors after cephalotripsy, to deliver the patient in extreme contraction, and that other instruments as tractors are necessary to aid the delivery.

4. That the Cæsarean section should not be performed when the contraction or deformity is as stated above, unless some other complications or circumstances exists, or presents.

THE RHEUMIC DIATHESIS IN DERMATOLOGY.

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In a paper upon the classification of skin-diseases, which I had the honor of reading before the New York Dermatological Society, December 8, 1874, and which was published in the *Archives of Dermatology*, April, 1875, I placed Eczema, Psoriasis, and Pityriasis, among the general diathetic affections of the skin, and referred them to a diathesis to which I gave the name of *Rheuma*. This name was chosen, firstly, in consequence of its etymological signification, which implies the idea of exudation; secondly, because the blood-condition underlying this diathesis is probably similar to, if not identical with that concerned in the production of rheumatism and gout; and, thirdly, because the vulgar name *Salt-rheum*, so commonly used in this country, embraces the affections under consideration. As the term Syphilides has been adopted to signify the cutaneous manifestations of Syphilis, and Scrofulides those of Scrofula, in like manner I have given the name of Rheumides to the cutaneous manifestations of the Rheumic diathesis.

THE RHEUMIDES.

The term Rheumides implies the existence of a constitutional condition or diathesis to which these affections may be properly referable. This, at the outset, involves the consideration of three important points. The first of these is, as to the veritable existence of such a diathesis; the second, as to its

nature; and the third, as to whether the affections which we have assigned to it really come within its influence.

Argument in support of the existence of the Rheumic diathesis would hardly seem to be necessary were it not for the fact that it is denied *in toto* by the German school; a school whose invaluable contributions to dermatology entitle its views to our highest respect.

Looking to the past, we find that from early times in the history of medicine there has been a more or less prevalent belief in the existence of a general condition intimately connected with certain cutaneous affections, and which was recognized by the Greeks under the name of *psora*. This term, though frequently used with great vagueness, still represented a prominent idea, and corresponded to the "*scabies*" of the Romans (Celsus), the affection to which the name *eczema* is to-day applied.

Paulus Ægenita included psoriasis, as well as eczema, under the term psora.

Rhazes[1] describes two kinds of "scabies," the moist and the dry, and places "pruritus" in intimate connection with them. He attributes them all to "*humores adustos*," and originating "*ex sanguine et phlegmate falso.*" The "scabies" here mentioned was equivalent to the ancient psora or modern eczema, and the "pruritus" probably corresponded to the lichen or papular eczema of the present day.

Leaving the distant past and coming to the dawn of modern systematic dermatology, we find Plenck[2] using the term "scabies" with very great looseness, making no less than eight varieties, of which but one, "scabies verminosa,"[3] corresponds to the affection now called by this name. The "scabies capitis" of Plenck, however, plainly includes eczema and "*est critica evacuatio humoris acrimoniosi, qui per glandulas capillitii excernitur.*"

Later we find that, instead of the Roman "*scabies*" or ec-

[1] Latin translation, Basileæ, 1544.

[2] "Doctrina de Morbis Cutaneis," p. 41, Viennæ, 1776.

[3] "Est Scabies, in qua Vermiculi seu Acari Reperiuntur," p. 42.

zema being the principal feature of psora, the modern scabies or itch proper, by a curious confounding of terms, became its chief synonym. The itch, then, became the representative of psora, and, although by most regarded as a local affection, was still by many believed to be of constitutional origin. This view was especially elaborated by Hahnemann,[1] and carried to such extravagant lengths that reaction was the natural consequence. The idea of the constitutional nature of the itch was finally overthrown by Renucci's[2] demonstration that the acarus scabiei was unquestionably the cause of the affection, and from that time the idea of psora as a constitutional disease no longer existed in the minds of the majority. Modern scabies was the parasite which destroyed the diathetic claims of the ancient and more respectable psora, and hence the German notion of the local nature of all these affections.

Turning to England, we find the original idea prevalent in the early part of this century. Parr,[3] speaking of psoriasis, says, "It is more strictly the *dry itch*, which, in compliance with authors, we have mentioned under the last article" (Psora). "It is always apparently connected with some disorder in the constitution, often with gout and rheumatism. The seminium of the disease is apparently in the constitution."

Later this use of the term psora became corrupted, as in Germany; and we find Plumbe[4] confounding it with parasitic scabies. The constitutional nature, however, of the affections which it formerly included, was not given up, and is at the present day steadily gaining ground in England.

In France we find a cutaneous diathesis, distinct from syphilis and scrofula, accepted almost without dissent; this diathesis is commonly known as the "*dartrous*," and, synonymous with dartre, we find a revival of the ancient term *herpes*.

[1] "Organon of Homœopathic Medicine."

[2] Gras, "Recherches sur l'Acarus," Paris, 1834.

[3] "London Medical Dictionary," American edition, Philadelphia, 1819.

[4] "Practical Treatise on the Diseases of the Skin," fourth edition, London, 1837.

Hardy[1] believes that the term dartres may with propriety be applied to a very natural family of cutaneous affections, possessing many common characteristics, to which he alludes. In general terms he describes those subject to this diathesis as "in appearance enjoying all the attributes of good health, but who are yet in a peculiar state which cannot be considered perfectly sound. Their integument is habitually dry, and perspiration is diminished. The skin is often the seat of lively itching, even in the absence of eruption. The appetite is generally well developed, and it is well known that the dartrous eat a much greater quantity of food than other patients in analogous conditions. Another important peculiarity is the extreme sensibility of the skin, and the facility with which it is influenced by the lightest and most fugitive impressions. Sometimes general excitement, alcoholic excess, watching, use of coffee, of certain kinds of food; sometimes a local excitement, irritating frictions, or the application of a plaster, will give rise to an eruption, often ephemeral, and not dartrous in character, but which reveals a particular predisposition of the economy, and the existence of a latent vice which needs but a favorable occasion to manifest itself." To this diathesis Hardy ascribes eczema, lichen, psoriasis, and pityriasis.

Gigot-Suard,[2] under the title of *herpetism*, includes the affections just mentioned, and, in addition, a few others whose claims to this position appear to me to be somewhat doubtful.

Bazin[3] separates the dartre or herpetis of Hardy and the majority of French writers into two principal diseases, which he calls respectively dartre and arthritis, and between which he endeavors to draw distinctions, which are in many cases so delicate as to be hardly appreciable. He adds to the list of affections a number which appear to be accidental rather than essential to either of these diatheses.

In Italy, where cutaneous diseases have been studied with

[1] "Leçons théoriques et pratiques sur les Maladies de la Peau," Paris.

[2] "L'Herpetisme," Paris, 1870.

[3] "Leç. théoriq. et cliniq. sur les Aff. cut. de nat. arth. et dart.," Paris, 1868.

great zeal and scientific care, we find a general acceptance of the herpetic and arthritic diatheses.

Coming finally to America, we find a very wide-spread belief in the existence of a constitutional condition manifested by certain cutaneous eruptions, which have received the common name of *salt-rheum.* It is this diathesis, equivalent to the *dartre* of Hardy, the *herpetism* of Gigot-Suard, the *herpetis* and *arthritis* of Bazin and Italian writers, and the *psora* of the ancients, for which I propose the term *rheumic* as a designation.

The existence of this diathesis cannot be considered completely proved, as the very nature of the case renders an absolute demonstration impossible. In this, as in most other theoretical questions in medical science, we are obliged to form our opinions by the preponderance of probability on one side or the other, and the ability of the theory to explain the observed phenomena. In favor of this diathesis, we have the concurrent opinions of many intelligent and experienced observers, running through long periods of time, and by its acceptance a means of explaining many occurrences which would otherwise be inscrutable.

The second question which requires consideration in this connection is the nature of the rheumic diathesis. This is not simply a matter of theoretical interest, but is of the utmost practical importance from a therapeutic point of view, since a correct understanding of the nature and etiology of the affections dependent upon it enables us to conceive and apply rational methods of treatment.

The older views upon this subject are not of much value, and even when we come to the present century we find very little clearly formulated. The English writers, as a rule, favor the idea that it usually depends upon the condition which gives rise to gout. Schönlein held that it, or at least one of its manifestations (psoriasis), was due to uroplania (an excess of certain urinary ingredients in the blood). This view Hebra expressly condemns. Hardy attributes the diathesis to a peculiar vice of the constitution, of the nature of

which he is ignorant; Bazin, so far as his arthritides are concerned, to the same general blood-conditions which predispose to inflammation of the joints, both rheumatic and gouty; Gigot-Suard to uric, sometimes to oxalic acid. It will be seen, then, that all the decided opinions which have been expressed, concerning the nature of the diathesis, by those who believe in its existence, are one in idea if not in words, and imply the existence of some *materies peccans* as the efficient cause of its manifestations. The views above stated are in the main based upon clinical observation, with the exception of Gigot-Suard's, which derives additional weight from the results of experimental investigation (detection of uric acid in the scales and secretions in these affections, and the induction of similar cutaneous lesions, by the ingestion of uric and oxalic acids).[1] My own view, derived from observation, study, and experiment,[2] harmonizes with those mentioned. It may therefore be formally stated that the affections pertaining to this diathesis are, in all probability, due to the accumulation in the blood of an excess of certain excrementitious substances, and presumably those which are also efficient in the causation of gout and rheumatism, with perhaps the addition of a few others whose relations to morbid conditions have not as yet received much attention. Although it is far from being susceptible of demonstrative proof, it is more or less probable that the following are the noxious agents, namely: uric acid, lactic acid, oxalic acid, creatin, creatinin, and possibly others. The first, fourth, and fifth of these, are always derived from preëxisting albuminoid substances; the other two, sometimes from albuminoids, and sometimes from substances belonging to the amylaceous and saccharine groups, and all of them represent either steps or side-products of the processes which bring about the metamorphosis of food into tissue, and that again into sub-

[1] *Op. cit.*, and "l'Uricémie," Paris, 1875.

[2] In the blood of two out of three psoriasic patients I found oxalic acid by dialyzing the serum. A dilute solution of chloride of calcium was placed in the outer vessel, and the result was a crop of octohedra, dumbbells, and "spherites" of oxalate of lime.

stances ready for excretion. Our present knowledge of physiological chemistry will not enable us to trace the exact processes and successive steps which lead to the formation of these bodies, but I think it will warrant the assertion that the general process is one of oxidation. In other words, that albuminoids (e. g., roast-beef) entering the body as food, finally leave it as urea, mainly through the instrumentality of oxidation, and, if the oxidation of the received albuminoids is incomplete, we have a diminished proportion of urea, and an increased proportion of uric acid, etc. This condition may be conveniently denominated, after Bence Jones,[1] one of *suboxidation.*

This incomplete oxidation appears to be, to a certain but limited extent, a normal condition, and suboxidized products are found in very small proportion in healthy blood, and ready for removal by the kidneys; and it is only when they accumulate unduly that they prove harmful. Some of these bodies are themselves, or form in the blood, compounds which are less soluble than the urea, and are not so readily excreted, and hence tend to accumulate. This accumulation occurs whenever renal action is deficient, although the production of the uric acid, etc., may be in normal quantity. Or, on the other hand, over-accumulation may occur from over-production, even when the kidneys are removing from the system the usual proportion of these excreta.

In the former case the kidneys are at fault, and the difficulty arises from either organic or functional disease of these organs, usually the latter. It is probable, however, that the over-accumulation is more frequently due to over-production than to deficient excretion. When this is the case it arises from one of two causes: first, deficient oxidation of a normal supply of ingested albuminoids; or, second, oxidation being normally active, it is still incapable of fully meeting the requirements of an occasional or habitual over-supply of peptones, and hence a quantity of only partially-oxidized and

[1] "Lectures on some of the Applications of Chemistry and Mechanics to Pathology and Therapeutics," London, 1867.

very insoluble products is left in the circulating fluid to be with difficulty excreted.

This duty the kidneys will perform up to a certain point, and for a certain length of time; but at last failing to be completely removed they seek other channels of exit, chiefly the bowels, but in part also the skin.[1] The bowels, being accustomed to the office of depuration, do not complain when any slight extra demand is made upon them; but the skin, less accustomed to the performance of this function, exhibits its impatience by *pruritus* and its rebellion by *eruption.*

If the supply of ingesta is normally and properly adapted to the body's needs, but oxidation is imperfect, we are compelled to seek deeper for a cause. It is to be found either in a deficient supply of oxygen in the blood, or, if the supply be hygienically sufficient, in a defective utilization of it.

This leads us to inquire how and where the general processes of oxidation are carried on in the body. Without stating the many theories which have been advanced in explanation of this process, I will simply offer the one which seems to me to have the greater probabilities in its favor, to wit, the one recently urged with so much force by Murchison.[2] This writer believes that the liver is the principal seat of the oxidizing processes, and that deficient functional activity of this organ is the *fons et origo* of most of the troubles arising from suboxidation. I have the more readily accepted the views of Murchison, as deductions from a different set of data had previously led me to suspect the liver of being intimately connected with the production of the rheumic diathesis. It is also probable that a certain amount of oxidation occurs in the tissues, and even in the blood itself.

Let us now return with the argument, and in the light of his theory trace a pound of beef from the mouth to the urinal.

[1] Gigot-Suard's "Experiments" (q. v.) seem to prove this. G. Bird ("Urinary Deposits," etc.) has observed eczematous eruptions "frosted" with crystals of urate of soda, and I have myself obtained uric acid from the sweat of rheumic patients. Lactic acid has been found in it by others.

[2] "On Functional Derangements of the Liver," London, 1874.

Entering the stomach it is acted upon by the gastric juice and changed into albuminose or peptones.[1] These are received by endosmosis into the portal capillaries, and are conveyed to the liver; here they wholly, or in part, undergo oxidation, and are conveyed thence by the hepatic vein to the vena cava, to the right heart, through the lungs, to the left heart, and from it to the general circulation, through the medium of which they are distributed to the tissues. Here, by further oxidation, perhaps, they become tissue, remain as such for a time, until by still further oxidation they are released from their morphological condition, and reënter the circulation, perhaps as urea, perhaps only as substances capable by still further oxidation of becoming urea, and ready for removal by the kidneys. If, now, these normal processes be anywhere obstructed, we have in the circulation the very insoluble products of deficient oxidation, which, unable to entirely escape by the kidneys, seek a vicarious exit, in part by the skin, and in so doing, give rise to the cutaneous troubles we are considering.

What causes the tendency to deficient oxidation by the liver and other organs concerned? This is a question which we cannot definitely answer. Excluding cases characterized by a deficiency of red corpuscles, anæmia, chlorosis, etc., in which the proximate cause is very evident, we come to others and by far the majority, concerning which we only know that sometimes the difficulty appears to be hereditary, and at other times acquired, and that in either case it is always difficult, and sometimes impossible to remedy, and that our efforts must be confined to controlling its results rather than to eradicating their cause.

There is, however, another important change in the constitution of the blood, and one which results directly from this over-accumulation of sub-oxidized product. Uric, lactic, and oxalic acids, combining with the free alkalies or alkaline carbonate existing in the serum, reduce its alkalinity—that is, render it sub-alkaline. Now, it is well known that processes

[1] I am considering simply the nitrogenous principles of the beef, not the fats, salts, etc.

of oxidation, whether within or without the body, are more readily accomplished in the presence than in the absence of an alkali; in other words, alkalies assist oxidation, and their diminished proportion in the blood-serum and the tissues greatly retards this normal process.[1] The importance of this fact, from a therapeutical point of view, will be immediately perceived.

This diathesis of sub-oxidation does not manifest its effects upon the skin alone, but also upon the mucous membranes and the joints; and, in all probability, underlies certain chronic organic lesions of the viscera. These, however, do not immediately concern us, and hence will not be specially referred to.

The third question which we are called upon to determine in connection with this diathesis is, the propriety of considering eczema, psoriasis, and pityriasis, among its dependents.

If these affections do depend upon this or any other diathesis or common constitutional condition, we should expect them to exhibit certain general characteristics indicating a mutual relationship. This they do, and the principal features which they possess in common, and which serve to point to this relationship, are the following:

They are not contagious.

They are frequently general; not, however, by simultaneous invasion of the surface, but by spreading from different foci.

They are frequently symmetrical.

They are usually chronic.

Their natural duration is indefinite.

They are obstinate, and do not readily yield to treatment.

They are frequently observed in different members of the same family.

They are frequently observed in different forms in different generations of a family.

Two or more forms may be present at the same time, or may appear successively.

[1] Bence Jones lays special stress upon this. A familiar example is the oxidation of sugar in Fehling's reaction, which will not occur except in the presence of an excess of alkali.

They do not always preserve their individuality, but sometimes merge one into the other.

Relapses are frequent.

They sometimes alternate with affections of other organs, especially of the pulmonary and gastric mucous membranes, and of the joints.

They itch.

The lesions are always superficial.

They never leave cicatrices.

They are more or less amenable to certain definite methods of treatment, which have little if any effect upon other cutaneous affections.

These many common features, together with the results of rational treatment based upon indications deduced from the supposed nature of the affections, tend, with increasing experience, to confirm rather than weaken the views which I have now for some years held concerning this diathesis, and the propriety of classing these affections among its manifestations.

Treatment.—Having now considered the nature of the rheumic diathesis, it remains to be seen whether we have any means at our command by which it may be counteracted, or its effects in any way modified. The measures which may be adopted for this purpose come under two heads, namely, rational and empirical. The rational treatment will be best understood by taking a retrospective glance at the morbid conditions present and their cause.

In the first place we have the blood surcharged with insufficiently oxidized excrementitious principles, less soluble than urea, the substance into which they would be changed if normal action were taking place.

2. The blood is sub-alkaline.

3. The accumulation in the blood of these excreta is due either to deficient action of the kidneys; or—

4. The kidneys acting normally, these substances are produced in excess.

5. This excess is due either to over-supply of albuminoid food, the surplus not being thoroughly oxidized; or—

6. The nitrogenous ingesta, not being excessive, there is failure on the part of the oxidizing processes to fully perform this function.

7. There are strong reasons for believing that the liver is the organ more particularly at fault in this connection.

The two principal indications, then, are to depurate the blood and to promote oxidation, and these we may expect to fulfill, with more or less success, by means at our command.

Depuration of the blood is to be effected by calling into more vigorous action either the kidneys, bowels, or skin. If the trouble is due to defective renal activity, a point which may be determined by careful estimation of the amount of nitrogen daily discharged in the urea, uric acid, etc., we must treat these organs with some leniency, and be careful not to urge them too much, since by undue stimulation we may increase the difficulties under which they are laboring, and defeat the very object we have in view, to say nothing of the risk of doing more serious damage. We will be obliged, therefore, to depend upon the skin and bowels for the fulfillment of the first indication. The functional activity of the skin is increased by exercise, bathing, and warmth; and is most rapidly and vigorously influenced by the hot-air or Turkish bath. This latter agent, in the absence of extensive eruption, is almost always of service, and, when properly managed, is not liable to be followed by injurious after-effects; it may, therefore, be applied frequently, even daily, with the happiest results.

If the bowels are to be stimulated, we may employ various cathartics, the most useful in this connection, perhaps, being the ordinary senna and salts, given in sufficient doses to produce one or two loose evacuations daily. Hardy praises very highly an infusion of wild pansy (viola tricolor), combined with senna, in about the following proportions:

℞. Violæ tricoloris,	℥ j.
Sennæ,	℥ ss.
Aq. bullientis,	O ij.
M.	

. One-quarter to one-half of this quantity to be taken daily, and the amount to be diminished gradually as the necessity for its employment lessens. Hardy states that he has given this purgative for two or three months at a time without ill effect.

In place of these remedies we may employ some of the natural mineral waters, as those of *Seidlitz*, containing sulphate of magnesia largely, without chlorides; of *Pullna*, characterized by its richness in sulphates of magnesia and soda, together with chlorides; or of *Friedrichshall*, containing both soda and magnesia, but less abundantly than Pullna. The native waters which seem to approach most nearly in chemical composition to those above noted are the *Estill* and *Crab Orchard* Springs of Kentucky.[1]

This active purgation, however, I believe is rarely required, the condition necessitating it (defective renal action) being the exception, and by no means the rule, as numerous quantitative analyses have indicated a normal excretion of nitrogen.[2]

If the kidneys are perfectly healthy, we may leave the bowels entirely alone, and call upon the former to perform most of the depurating work. This is effected by diuretics, and the ones specially serviceable in this connection are vinum colchici, infus. digitalis, vinum caffeæ viridis,[3] balsam of copaiba, propylamine, carbonate of lithia, and vichy.

[1] We know so little concerning the native mineral waters, that it is difficult to speak confidently as to their effects. Many of them, especially those of this State (New York), contain a large proportion of lime (sulphate and carbonate), which does not appear to me to be a desirable ingredient, except, perhaps, in certain special conditions not connected with skin-diseases.

[2] This statement is chiefly based upon urea determinations. The quantitative analysis of uric acid being tedious, and often unsatisfactory, has not been so frequently employed.

[3] I first learned the value of this preparation in Gigot-Suard (*op. cit.*), and have it prepared as follows: One pound of ground unburned coffee is added to a quart of good sherry, and left to digest for two weeks. The mixture is then filtered through flannel, and the residue subjected to pressure, and enough fresh wine is added to make a quart. Dose, ℥j or ℥ij *per diem.* This wine is prepared for me by Mr. B. W. Dyer, No. 460 Fourth Avenue.

These remedies, one and all, appear to exert a marked influence upon the urine, notably increasing the amount of solids daily excreted in this fluid. They are, moreover, among our chief reliances in gouty and rheumatic conditions generally. The carbonate of lithia and vichy, besides being diuretic, tend in addition to restore the normal degree of alkalinity to the blood, and by their presence as alkalies to assist oxidation. These different diuretics may be used singly or combined, and for a considerable period. Lithia and vichy, however, and alkalies generally, if too long employed, tend to impoverish the blood by diminishing the number of red corpuscles. To obviate this, the use of the benzoate of lithia, combined with iron, has been suggested. A better plan, however, if we anticipate a prolonged use of alkalies and other diuretics, is to intermit their employment for one or two weeks out of each month, giving iron if necessary in the intervals.[1]

Having put in force the measures necessary for the depuration of the blood, and the reëstablishment of its normal alkalinity, attention must be directed to the question of oxidation.

If the conditions present be simply due to incomplete oxidation of an excessive amount of albuminoid ingesta, the course is very clear. It is only necessary to diminish the proportion of this kind of diet. In other words, cut off the meat to a greater or less extent, and substitute for it a larger quantity of bread, vegetables, and fats. Some of these patients are exceedingly fond of meat, and eat it in large quantities, and are sometimes inclined to rebel against restriction of their diet. The quantity of meat eaten by many persons is greatly in excess of the real bodily needs. And this excess being of no service is pretty apt to do harm, and soon brings the patient in contact with his physician. In these cases, then, our principal effort should be to induce the patient to modify his diet in the way suggested, and, even if he is a little rebellious at first, it is surprising how soon he becomes reconciled to the

[1] It must be remembered that we are dealing with chronic conditions, and treatment to be effectual must be continued for a long time.

changed conditions, and frequently loath to return to his former habits.

If, on the other hand, but a moderate quantity of nitrogenous food is ingested, and even this is incompletely oxidized, it will be necessary to institute measures specially designed to increase oxidation. The red corpuscles being the vehicles by which the inhaled oxygen is distributed to the different parts of the body, it is, of course, of the first importance that they should be present in normal quantities. Any notable deficiency in this respect is easily ascertained, and may usually be remedied to a great extent by the use of preparations of iron. This being accomplished, we must endeavor to insure a full supply of oxygen by exercise in the open air, good bedroom ventilation, and the like. In addition we may attempt to furnish oxygen directly to the blood by inhalations of the pure gas, or better, perhaps, by inhalations of oxygen, a small portion of which has been rendered more active by ozonation. Further, we may employ certain medicines which contain oxygen largely, and are believed to be capable of giving it up to the blood, as, for instance, the chlorate of potassa. The alkalies already mentioned, which by their presence assist oxidation, are appropriate adjuvants.

Finally, if the liver be torpid—that is, functionally inactive—we may have recourse to the occasional, and, in some cases, frequent use of cartain drugs which have the reputation of being hepatic stimulants, as mercury, podophyllin, etc.

The above outline of treatment is certainly the one which the conditions supposed to exist would naturally suggest. I should hesitate, however, to offer it, even with personal experience in its favor, were it not that there is abundant corroborative testimony from other sources in favor of each and every one of the remedies mentioned. The value of cathartics, of diuretics, of alkalies, of chalybeates, and of hepatics, as isolated remedies in the affections embraced in this diathesis, is recognized by almost every modern writer, and their employment counseled under various circumstances. Heretofore their use has mainly been empirical, and not founded

upon preconceived views as to the special indications which they fulfilled.

Their acknowledged clinical value, however, is strong presumptive evidence of the at least approximate correctness of the theoretical views that have been expressed, and should induce us to seek further for remedies still better adapted to fulfill the indicated requirements.

It is not of course supposed that in any given case all of the drugs mentioned will be required, but the happiest results are to be expected from their judicious selection, combination, and alternation.

The treatment which I have here advocated for the affections belonging to this group is intended to replace the method which, until recently, has received almost universal adhesion. I allude to the treatment by arsenic.

Arsenic has been and is still by many, perhaps by most, regarded as the sheet-anchor in the management of these affections. Its reputation is based upon the fact that it has the most undoubted control over many of the manifestations of this diathesis; a control evidenced by the prompt removal, in many cases, of the visible lesions and other appreciable symptoms. But does it, in addition to this, exert any influence upon the constitutional conditions which underlie them? Does it in the slightest degree tend to prevent their relapse? I have never been able to perceive that it did. In my earlier experiences I employed arsenic largely, and obtained the effects usually ascribed to it; gradually I used it less frequently, and at present prescribe it but seldom, and have no hesitation in saying that the arsenical treatment of these affections, though often more prompt, is on the whole less satisfactory than the method which I have here detailed.

The foregoing refers simply to the internal treatment of the rheumic affections, but it must not be supposed that dependence is to be placed upon it alone. On the contrary, local treatment is of service in almost every case, but can only be considered in connection with the special affections themselves.

HOSPITALS: HISTORY OF THEIR ORIGIN AND DEVELOPMENT—THEIR PROGRESS DURING THE CENTURY OF THE AMERICAN REPUBLIC.

By W. GILL WYLIE, M. D.

Read February 16, 1876.

It is the common belief that hospitals, for the treatment of the sick poor, are the offspring of Christianity; that, previous to the Christian era, no such institutions as our hospitals ever existed. The best encyclopædias, both English and American, make no reference to the fact that hospitals were known previous to the coming of Christ, and not infrequently we hear men in public addresses making the same mistake. Beyond question, hospitals for the purpose of treating the sick poor were founded in India, among the Buddhists, several hundred years before the advent of Christ.

During the reign of Asoka, who died 226 years B. C., the Buddhists cut on rocks their edicts on hospitals, one of which, dated 220 B. C., can be seen to-day.

It directs that, along the routes of travel, hospitals shall be erected, that they be "well provided with instruments and medicines consisting of mineral and vegetable drugs, with roots and fruits," and also that "whenever there is no provision of drugs, medicines, roots, and herbs, are to be supplied and skillful physicians are to be appointed to administer them, at the expense of the state."[1]

In Mr. Wise's *Review of the History of Medicine* (vol. i., page 392) there is a quotation from Tounour's translation of the "Mahawauso," stating that "Buddha has appointed a

[1] Review of the History of Medicine. By Thomas A. Wise, London, 1867.

physician for every ten villages on the high-road, and built asylums for the crippled, the deformed, and the destitute. His son, Upatiso, built hospitals for cripples, for pregnant women, and for the blind, and diseased, and Dhatusend built hospitals for cripples and for the sick."

At the present time there are no hospitals for treating human beings among the Buddhists. It seems that they were provided by the state, and disappeared with the government; but hospitals for diseased, old, and crippled animals, which from the earliest period were founded and supported by private benevolence, are still kept up.

Scavoneur, in his "Voyages" (vol. ii., page 489), gives an account of the Banian hospital which still exists at Surat, at the time of his writing, about one hundred years ago:

"This curious institution was supported by the one anna per cent. on the rupee of the merchant's clear gain, to which were added the fines of certain venal offenses, under the supervision of the chief Banians. In 1770, when trade had decayed, the revenue was upward of six hundred pounds a year, and so careful were they of the animals, that bread-and-milk were provided for two which could not crop grass. The hospital consisted of twenty-five acres, surrounded with a high wall, divided into courts and defended by sheds and yards for the accommodation of the animals."

"Not far distant on a rock, in Guzerat, are the edicts of Asoka, cut on it more than two thousand years ago." Hamilton in his "*Hindostan*" (vol. i., page 718), tells of an old tortoise, that was seen in one of these hospitals, that had been there for seventy-five years.

It is said that "the sick were treated in the temples of Æsculapius, 1134 years B. C., at Titanus, a city of Peloponnesus," but there is no evidence that these temples were used for the purpose of treating the sick poor; still they were in some respects similar to our hospitals.

Dr. John Watson, in his *Anniversary Discourse before the New York Academy of Medicine*, says: "As asylums, these temples bore no inapt resemblance to the hospitals and in-

firmaries of modern times; into which, in fact, some of them were ultimately converted. The temples of Æsculapius, Cos, and Tricca, according to Strabo, were always filled with patients, and along their walls the tablets were suspended, upon which were recorded the history and treatment of the individual cases of disease."

The valetudinariums of the Romans, referred to by Seneca and other writers, and which are defined by Andrews, Ainsworth, Cooper, and other authorities, as places where men lie being sick, sick men's lodgings, infirmaries, or hospitals for sick folk, a sick-room, must have been institutions somewhat similar to hospitals. As they are not described by any of the many Roman historians as public institutions, they were probably something like our private hospitals—water-cures.

There was a public building on an island of the Tiber at Rome to which slaves were removed when sick, and one at Delos used as an asylum for aged women, and near some of the temples houses were erected for those sick while visiting them.

In the East caravansaries or resting-places for travelers have existed from time immemorial. In all of the early pilgrimages some such resting-places were customary, and were usually found near the shrines and temples.

After the birth of Christ, when his words began to reach the hearts of men and lead them to acts of kindness, and when Jerusalem and the roads approaching it were crowded with pilgrims, special accommodations for the use of those taken sick were established in connection with these caravansaries. Afterward, when monasteries and convents were established, they were the resting-places of pilgrims, and in these we find there were special apartments arranged for the sick. In the East, in the time of the Emperor Julian, these apartments for strangers and paupers in the monasteries were called *xendochia.* Among the Romans it was the custom to set aside apartments in their houses for guests which they

[1] Cowper, in London *Daily News*, October, 1875.

called *hospitalia* and it is from this that our word hospital is derived, and it was from the spirit of charity taught by Christ, stimulated by the necessities of the pilgrims, that our first hospitals originated.

As early as A. D. 300 it is said that several hospitals were established for the use of sick pilgrims near Bethlehem, under the direction of St. Jerome, in whose writings the term "hospital" first appears in connection with curative establishments.[1]

Besides the necessity of providing places of shelter for sick pilgrims, another cause that influenced the founding of hospitals and other charitable institutions was, that the barons of the middle ages did not distribute corn among the poor, as was the custom with the Romans, and when Christianity was introduced into the Roman Empire many slaves were set free and became helpless paupers.

"To either St. Ephraim, who died in 381, or St. Fabiola, is due the credit of founding infirmaries, which were supported by charitable contributions for the exclusive purpose of treating the sick."

Hospitals were spoken of "in the Council of Nice, A. D. 325, as institutions well known and deserving support and encouragement."[2]

The famous Hospital of Cesarea was founded A. D. 370. The Hôtel Dieu, perhaps the oldest hospital in Europe now in use, was founded about A. D. 600 by St. Landry, Bishop of Paris, for all sick and destitute persons of whatever sex, age, or condition. Its motto was "Medicus et Hospes"—sick pilgrims and mendicants all were received.

The first hospital built in England especially for the sick was erected at Canterbury by Archbishop Lanfranc. In 1070 and in 1208 the Hospital of the Holy Ghost and St. George's Hospital were established in Berlin. After this a hospital was considered requisite for every city to afford shelter to the poor and sick.

[1] Dr. Toner's "Contributions to the Annals of Progress in Medicine."

[2] Ibid.

The introduction of the leprosy during the Crusades, and later epidemics of small-pox and cholera, necessitated the separation of the sick from the well, and to accomplish this many lazarettos were built. Lamothe, on Charitable Legislation, says that, in 1226, there were two thousand such institutions in France alone.

In 1048 a school of medicine was established at Salerno, and the same year an order, called the "Brethren of the Hospital," for the care of the sick in Jerusalem, dedicated to John the Baptist, was organized, and in 1118 knightly vows were added. After this brotherhoods of Hospitallers were formed and extended through Europe.

As the Crusaders increased, the brotherhoods became more numerous and richer; and it was not uncommon for rich persons, when dying, to bequeath their property to the hospital in which they had been relieved while away from home.

The hospitals established by these brotherhoods were always near and connected with a monastery or cathedral. Of their internal arrangements but little is known.

It seems that the knights or brothers bound up the wounds, and acted as physicians. The earliest account of physicians or surgeons being connected with a hospital among the Templars was under John de Lastic, who, in 1437, defined the duty of the physicians and surgeons.[1]

In 1456 the Grand Hospital of Milan was opened. This remarkable building is still in use as a hospital, and contains usually more than two thousand patients. "The buildings stand around square yards, the principal one being much larger than the others, and separates the hospital into two parts." The main wards on either side of this large court form a cross, in the centre of which "is a cupola, with an altar beneath it, where divine service is performed daily in sight of the patients."

These wards have corridors on both sides, which are not so lofty as the ceilings of the wards, and consequently there is plenty of room for windows above these passages. The

[1] Beckman's History of Inventions.

ceilings are thirty or forty feet high, and the floors covered with red bricks or flags.

The outside wards are nothing but spacious corridors.[1] The wards were first warmed by open charcoal braziers.

This hospital, built at the time when the Church of Rome was at the height of her power, and but a short time before the Reformation, is a good example of what had been attained toward the development of hospitals, and it shows how much a part of the Church the institution of hospitals was. A close study of all hospitals built subsequent to this period for two hundred years on the Continent of Europe and about one hundred years in England, in fact just so long as the Church of Rome had control of the hospitals, shows that no progress in the construction of hospitals was made.

In all of these hospitals the first idea in the erection was to make the hospital a church institution, and on this account the wards were clustered about the chapel. Add to this idea of making the hospital a *church institution*, that of *economy* and *convenience of administration*, and take the plan of the Grand Hospital of Milan as a model, and we have the source of our modern block plan and corridor hospitals.

About the first of the sixteenth century another cause began to influence the establishment of hospitals, namely, the necessity of providing for the care of the large numbers of sick and wounded incident to war. From the earliest ages it seems to have been the custom to have a surgeon or physician in attendance on an army, but, previous to the times of Ambroise Paré (1560), there is no evidence that the physician or surgeon was considered a necessity or of much consequence in military affairs.

As there was no special provision for the wounded after a battle, they were distributed in the nearest towns and quartered upon the inhabitants. There are one or two referenced by ancient historians, showing that the wounded were sometimes placed in tents, but there is no account of such a thing as a military hospital. The soldiers were taught to dress their

[1] Hospitals, Infirmaries and Dispensatories. By F. Oppert. 1867.

wounds with oil and wine, and in many cases left to take care of themselves when wounded.

The Italians were the first to produce writers on military surgery about the beginning of the thirteenth century. Early in the fourteenth century the study of surgery was introduced into the Universities of Montpellier and Paris, but we find no account of a military hospital being erected, until 1575 when at the siege of Metz, through the influence of Ambroise Paré, the first famous military surgeon, one was built.[1]

Thus we see that the spirit of humanity without the aid of science did not make proper provision for the sick and wounded soldiers. Three hundred years later, when science had developed into such proportions as to divide the control of the world with religion itself, we shall find the experience afforded by the large number of sick and wounded incident to wars to be by far the most important measure of all others in bringing about reforms in hospitals.

In 1670 Louis XIV. began to build the Hôtel des Invalides. Beckman, in his "*History of Inventions*," speaking of this hospital says, " The extravagant magnificence of which is rather a proof and monument of the profusion and pride of that sovereign than of his care for meritorious soldiers."

This is one of the first instances of this personal monumental idea showing itself in the construction of hospitals. To-day, when so many hospitals are founded by individuals, the monumental idea has become a serious hinderance to the adoption of that plan of building known to be the best for the purpose of curing the sick.

One of the first houses established for the sick poor was that built at Rome by Fabiola, a Roman lady, the friend of St. Jerome.[2] But this and all other hospitals founded by individuals were made church institutions until the beginning of the eighteenth century, when several hospitals, as Guy's, were founded in England by individuals, and the monumental idea

[1] On the Establishment of Army Hospitals, by Edward A. Crane, M. D.

[2] Beckman's History of Inventions.

began to show itself in the style of building; since then many have been built, and in almost all the idea of making a monument has been the first and most important consideration in selecting the style of building, and has lately done much to hinder the adoption of the best plans so far as the welfare of the patients is concerned.

The Influence of Science on the Development of Hospitals. —Before the Reformation in the sixteenth century, hospitals, lazarettos, and all other charitable institutions, were completely in the hands of the ecclesiastics, and had become a useful part of the machinery of the Church of Rome, which then in a great measure directed all the affairs of Europe.

Until this time the knowledge of medicine and pretty much all learning was confined to the priesthood. In fact, until the eighteenth century almost all hospitals were more like our poorhouses than the hospitals of to-day. At this period positive science was established, but the practical adaptation of science or inventions was only just fairly begun. Curative medicine was hardly in advance of the time of Hippocrates, and sanitary science was in its infancy.

Early in the seventeenth century, it is said that Gustavus Adolphus of Sweden established the first regular military sanitary service, but we have found nothing to show that sanitary science to any extent influenced the construction of hospitals, certainly not civil hospitals, before the eighteenth century. After the Reformation, during the enlightened reign of Elizabeth, on account of the prevalence of the plague, efforts were made to bring about improvement in dwelling-houses, by limiting the number of inmates in each house, etc., and in the seventeenth century the fearful ravages of both the cholera and jail or typhus fever excited a general interest in efforts to prevent the spread of these contagious diseases.

Toward the latter part of this century, the writings of Sydenham did much to improve the science of medicine, and early in the eighteenth century we find several authors treating of air intelligently. Among these were Hoffman and

Beucrus. Arbuthnot wrote a valuable treatise on air, making use of the laws of physics.

In 1713 Cardinal de Polignac wrote "Le mécanique à Feu," in which he treats of ventilation, and makes use of the laws of physics in constructing fireplaces, and to him is due the credit of inventing the first machine for forcing ventilation. He says: "It is not the warmth of a room, but its inequality of temperature and want of *ventilation*, that is the origin of numerous maladies; we cannot, therefore, be too cautious of living in those in which this dangerous impurity of heat and motionless air exists, nor remain too long in rooms into which temperately warm *fresh* air is constantly flowing and diffused."[1]

The cardinal's method of heating the air before it enters the room, by ducts and chambers behind the fireplaces, in which he makes use of the laws of physics, mathematics, etc., is most scientific and ingenious.

In 1715 Jean T. Desaguliers, a French refugee then residing in London, translated the work of Cardinal de Polignac (M. Gauger) into English. In 1723 Desaguliers was requested to improve the ventilation of the House of Commons, which before that time had been ventilated by means of four holes in the ceiling connected with hollow pyramids to take off the foul air—but often cold air would descend.

Desaguliers very ingeniously arranged a fire with flues to heat the air in the pyramids or shafts, and thus ventilate the room. In 1727 he designed a machine, which by means of pumps forced the fresh air in, and the foul air out of mines. In his "Cour de Physique," vol. ii., page 474, he describes a ventilator which he had proposed to the Royal Philosophical Society for "changing the *air* of the rooms of sick people in a little time, either by drawing out the foul air or forcing in fresh air, or doing both successively without opening doors or windows," which he thought would be of great use in all hospitals and prisons, and also serve to convey air into a distant

[1] Bernan's History and Art of Warming and Ventilating, vol. ii., p. 3, London, 1845.

room.[1] The motion was given to the air and the current established by means of a number of fans revolving in a circular case—the exact principle of the fan-ventilator of to-day.

It was not altogether original, but the application and improvement of the plan proposed by Cardinal de Polignac, and is "something like Popin's Hessian bellows." This centrifugal wheel-ventilator was applied to the House of Commons in 1736, and used for seventy-five years. In 1741 Samuel Sutton published a plan for drawing off the foul air of ships by means of pipes passing through the fires and connecting with the flues, thus by suction forcing out the foul air. And about the same year, 1741, Stephen Hales published the description of a ventilator that worked on the principle of the bellows.

During the first half of the eighteenth century, in England there is but little to be found in medical works having a direct bearing upon hospital reform, but there were several important hospitals built; among these was the Royal Infirmary of Edinburgh.

The medical school in connection with the university, led by Alexander Munro, had grown in importance, and, as there was no hospital then in Edinburgh, the professors and those interested in the school succeeded in establishing the Royal Infirmary in 1734. The doctors took a deep interest in the erection of the building, as they were at that time the most famous in Britain; the plan adopted by them will give a good idea of the progress that had been made in hospital construction, and is interesting as being one of the first hospitals erected with the design of subserving the interests of a medical school.

The original building is still standing; it consists of a central administrative portion with wings two stories high on either side, containing the wards; the whole hospital is one solid building, but the windows are large and opposite, and the number of beds in each ward is small compared with the old church hospitals, and in many respects it shows progress,

[1] Philosophical Transactions, and Bernan's History of Ventilation.

but it is evident that the controlling ideas in the plan of construction were *economy* and *convenience of administration.*

Comparing this hospital with those of Europe of the same date will show that England, long freed from the yoke of the Romish Church, had made more progress in hospital reform than any other nation.

Early in the eighteenth century, through the writings and teachings of Boerhaave, the Medical School of Leyden became the most famous of that period, and held the position of the first medical school of Europe for a generation.

Boerhaave was a man of great erudition, and it is difficult to say to what extent his teachings are original; at any rate many of his aphorisms will bear the light of science to-day, and show a comprehensiveness that extends to some of the latest scientific discoveries and methods of treating disease. The following two aphorisms have been selected as having a direct bearing upon the subject we have in hand: Aphorism 245: "But it is more especially serviceable, in the beginning of the cure, to make an exact closure of the wounded lips while bleeding, and making the dressings seldom, and very expeditiously, carefully defending the parts in the mean time from everything too moist, oily, or relaxing, and also from the *air itself.*" Aphorism 200, on wounds in general: "The air of the patient's chamber should be always pure and free from putrid exhalations; that which is dry, and moderately warm or temperate, is best; and it should be frequently renewed or changed."

We will not give our explanation of these words, but that of one of his pupils, Gerard van Swieten, which we find in Van Sweiten's "Commentaries on the Aphorisms of Boerhaave," published more than a hundred years ago. On page 181, vol. ii., after giving the above aphorisms, he says: "Where a great number of wounded patients lie together in a hospital, the air is filled with putrid exhalations, which affec- all of them, and kill many who otherwise might have been preserved; such places should therefore have the windows often opened, and the air changed or blown out, to remove the

putrid exhalations. It is indeed often advised to perfume the place for that purpose; but changing of the air is much more serviceable to the diseased."

"But above all, those patients are observed to suffer most for want of fresh air, who have wounds in the head, as we are assured from observation." Can anything we have to say to-day express more clearly the danger of foul air and the necessity of pure air in treating wounds successfully? If Boerhaave did not originate these ideas, he at least by his learning and wisdom collected them, shaped them into living germs, and planted them in the minds of his pupils who were destined to become the first and prime movers in the development and practical application of sanitary science in treating the sick.

Among the pupils of Boerhaave at Leyden, were Sir John Pringle and Gerard van Swieten; Donald Munro and Richard Brocklesby were also students at Leyden; all four are noted as being among the first men who wrote and taught the application of sanitary science in the construction and management of military hospitals.

Of books that had a wide circulation we have been able to find but few that treat of the practical application of sanitary science in caring for the sick and wounded, before the publication of the writings of Sir John Pringle, and none that will compare with the advanced views taught by him. In 1750 he published a scientific essay on antiseptic substances (probably the first on that subject), and in 1752 the first edition of his famous work "Observations on the Diseases of the Army" appeared. On page 86, in speaking of the causes of disease in armies, he says: "The last source is from hospitals, barracks, transport-ships, and in a word, from every crowded place where the air is so pent up, as not only to lose part of its vital principle by frequent respiration, but also to be corrupted by the perishable matter of the body, which, as it is the most volatile part of the humors, is also the most putrescent," etc.

And again he says: "As to the disposition of hospitals with

regard to preserving the purity of air, the best rule is, to admit so few patients into each ward that a person unacquainted with danger of bad air might imagine there was room to take in double or triple the number." And when speaking of hospital and typhus fever, he recommends barns and other open buildings in preference to closed houses, and says that pure air is of more importance than warmth, and adds, "It may be received as a maxim that the more fresh air we let into hospitals the less danger there will be of breeding this dangerous distemper." He recommends open fires as the best method of heating, and says soldiers may be treated under sheds.

About this time, science was making rapid progress in all directions, and there seems to have been a general awakening to the necessity of sanitary reform.

In a small volume of Van Swieten's, published in 1760, he gives the following aphorism: "The lodging of a number of men in a place wanting in space should be avoided with the greatest care; but should it at any time become necessary, the air must be renewed there as often as possible, whether the men who are lodged together are well or sick, for it is from a want of ventilation that the most dangerous diseases arise, including even those which are contagious."[1]

In 1764 Donald Munro published a work "On the Condition of Military Hospitals." In this he proposed the application in hospitals of Sutton's method of ventilating by means of pipes or ducts opening in the ceiling of the wards and connecting with the flues of the fires, and he also recommends the use of large sheds or wooden huts, which he says have been tried by Richard Brocklesby, and found to be the best buildings for treating the sick of the army and navy.

Long before the time of Brocklesby no doubt wooden huts had been used for sheltering the sick during epidemics, from necessity. The object being to separate the diseased from the well, without any design of placing the sick under the most favorable circumstances to recover.

In the same year, 1764, Richard Brocklesby published his

[1] Crane On the Establishment of Army Hospitals.

"*Economical and Medical Observations.*" In an essay on the most effectual means of preserving the health of seamen, by James Lind, M. D., published 1762, in London, we find the first definite proposal on the classification of patients ; he says ; "In all hospitals, there should be separate wards allotted for different diseases, and the foul wards in a hospital ought always to be the best aired, and, where the contagion is eminently malignant, spacious tents with fireplaces are greatly preferable to any close ward or apartment, for dissipating infection and for the recovery of the diseased."

Although the general interest in sanitary affairs and the views and practical suggestions of the above-mentioned works undoubtedly brought about important reform in the management and internal arrangements of the English hospitals, yet no hospital was built on a different plan or that could be considered an improvement on the plan of the Royal Infirmary, except perhaps one that was built at Stonehouse, near Plymouth, England.

We have not been able to get a full description of the plan of this hospital ; we know that it was commenced in 1756 and finished in 1764, and used for sick seamen.

The wards were in separate pavilions, with only twenty beds to the ward, and the windows were opposite. This was in all probability the first "pavilion" hospital; the name of the architect was Roverhead, of London. It was at the time considered a model hospital, and was visited by members of the committee appointed from the French Academy of Sciences in 1786, to report upon a plan with which to replace the Hôtel Dieu, and from its plan of detached buildings or pavilions they took ideas which guided them in preparing their famous report.

The improved plan of this pavilion hospital at Stonehouse was not adopted in England, nor do we know of any hospital built in England on this plan until 1860, when the Blackburn Infirmary was built near Manchester, and this was but a reflection of the Stonehouse plan from the Lariboisière Hospital in Paris, which was finished in 1854 after the plan suggested

by the French Academy committee in 1786, fifty-eight years before.

In the writings of Pringle, Brocklesby, Munro, and Lind, and in the building of the Pavilion Hospital at Stonehouse, the spirit of sanitary reform which began with the Reformation seems to have reached its highest point of development in so far as the construction of hospitals was concerned, and except in matters of internal arrangements no mprovements were made, and interest in the subject of hospital construction seems to have lain dormant for a century.

In Europe up to 1776, a century ago, we find nothing of interest to record as an improvement in hospitals. The progress of science up to that time practically had not reached the subject of hospitals. The civil hospitals were still in the hands of the ecclesiastics, and even in Paris, then the greatest scientific centre of the world, they were, according to the accounts of numerous authorities, in a shocking condition.

We have traced the development of hospitals in Europe up to the time of the Revolutionary War. We will now turn to America.

Prescott, in his "*Conquest of Mexico*," vol. i., p. 48, speaking of the Aztec civilization, says: "I must not omit to notice here an institution the introduction of which, in the Old World, is ranked among the beneficent fruits of Christianity. Hospitals were established in the principal cities, for the cure of the sick, and permanent refuge for the disabled soldiers; and surgeons were placed over them 'who were so far better than those in Europe,' says an old chronicler, 'that they did not protract the cure, in order to increase the pay.'"

Mr. Prescott gives, as his authorities for the above statements, "Torquemada, Monarch. Ind., lib., xii., cap. 6; lib. —, cap. 3;" "Ixtlilxochitl, Hist. Chich., MS., cap. 36." On page 48, of the same volume, he says, in speaking of Montezuma, "He showed a similar munificent spirit in his public works, constructing and embellishing the temples, bringing water into the capital by new channels, and establishing a

hospital, or retreat for invalid soldiers, in the the city of Colhuacan."

In answer to certain questions, General J. W. Phelps, an authority on the subject, writes: "There is abundant evidence to show that the Aztecs came from Eastern Asia, and that their civilization originated in Buddhist ideas." Like the Buddhists, their knowledge of medicine was considerable; but their religion, so unlike that of the Buddhists, could not have prompted the building of hospitals, for they are supposed to have sacrificed and eaten one in a thousand of their population every year, as a religious institution. It is possible that the idea of the hospital may have been directly derived from the Buddhists, and that it was prompted by their religion before it sank to cannibalism.

"After America was settled by Europeans, it is said that a small hospital was established at Quebec as early as 1639."[1]

The first account of a hospital in the territory now known as the United States is given by E. B. O'Callaghan, in his 'New Netherland Register.' In speaking of a hospital in use on Manhattan Island in 1658, he says: "This hospital was established, at the request of Surgeon Hendrickson Varrenvager, for the reception of sick soldiers—who had been previously billeted on private families—and for the West India Company's negroes. In 1679 this hospital consisted of 'five houses.'"[2]

Early in the eighteenth century pest-houses were established at Salem, Mass., New York, and Charleston, and in 1717 a hospital for contagious cases was built at Boston, Mass.

In 1750, Dr. Thomas Bond originated a movement to build a hospital in the city of Philadelphia. Dr. Bond, in his efforts to bring the matter before the people and to secure a charter from the provincial government, was assisted by Benjamin Franklin, at that time a printer in Philadelphia. The charter was granted on the 6th of May, 1751; temporary

[1] Dr. Toner's Contribution to the Annals of Progress in Medicine.

[2] J. W. Beekman, Centennial Address, New York Hospital, 1871.

buildings were used till 1755, when the corner-stone of the present Pennsylvania Hospital building was laid, but the original plan was not completed until 1805.

The hospital, built on the original plan, is now in use. It consists of a central administrative part, with two wings of wards two stories high, with a basement.

Considering the early date at which the plan was adopted, it was a very good one, and far surpasses the old monastery and convent buildings that constitute a large part of many of the most renowned hospitals now in use abroad.

This was the first chartered hospital of this country. In 1775 four hundred and thirty-five patients were admitted into it.[1] The scientific and philosophical works of Benjamin Franklin had a marked influence in educating the people of this country in the practical application of the teachings of sanitary science, and from his connection with the Pennsylvania Hospital, and his writings on ventilation, stoves, etc., it seems that to him is due the credit of influencing the selection of so good a plan.

Mr. Beekman, in his "Centennial Address" before the Society of the New York Hospital, says:

"In 1771 the city of New York was a small town of about twenty-one thousand inhabitants, scarcely extending on the north as far as St. Paul's Church.

"The governors of King's College (now Columbia) had established a medical school on the 17th of September, 1767, and to the exertions of two of the professors, Dr. Samuel Bard and Dr. John Jones, the New York Hospital owes its origin.

"The charter was obtained in 1771. The governors empowered Dr. Jones in 1772, then intending to sail for Europe, to make collections of money and to buy medicines and apparatus abroad, and the next year decided to build the hospital on a plan proposed by Dr. Jones on his return.

"On the 6th of March, 1775, when hardly finished, the hospital was completely destroyed by fire, and was not rebuilt until 1791. In 1775 Dr. Jones published a little book under

[1] Dr. G. B. Wood, Centennial Address, Pennsylvania Hospital, 1851.

the title '*Plain Concise Practical Remarks on the Treatment of Wounds and Fractures, to which is added a Short Appendix on Camp and Military Hospitals: Principally designed for the Use of young Military Surgeons in North America.*'" This little book was very useful during the Revolution.

The first pages of the Appendix relate to all hospitals, and the whole subject of hospital reform is concisely expressed. In a foot-note an outline of his plan for the New York Hospital is given. As the advanced views expressed in these remarks on civil hospitals, illustrated by the few words on the plan, antedate by more than ten years any other publication we have been able to find, we will quote the following:

"Among the variety of public errors and abuses to be met with in human affairs, there is not one, perhaps, which more loudly calls for a speedy and effectual reformation than the misapplied benevolence of hospitals for the sick and wounded.

"We daily see persons of every rank and sex contributing to these charities with a spirit of liberality which does honor to humanity, while many of them, with the most becoming zeal, are devoting their time and sacrificing their private interest to the care of superintending the structure and management of the house; and yet an absurd, mistaken economy has hitherto not only rendered all this pious labor and expense, in a great measure, useless, but even fatal and destructive to the very end and aim of the intended purpose—that of healing the diseases of the sick and poor.

"To those who are unacquainted with the subject in question, it will doubtless appear a very extraordinary assertion that there is not at present, in the capital of the kingdom, a single hospital constructed upon proper medical principles; yet, it is a fact very generally acknowledged by the most eminent men in the profession of physic and surgery in England.

"If we inquire into the cause of such glaring absurdities (p. 82), we shall easily trace them to those sources of darkness and ignorance from which most of our civil and religious abuses have originated; but how they should be continued,

to disgrace the improvements of more enlightened times, can only be resolved by reflecting on the pride, obstinacy, and self-interest which are too generally annexed to ancient errors.

"If great and populous cities have been justly styled the graves of the human species, the large and crowded hospitals generally built in them may, with equal truth and propriety, be denominated the lazarettos or pest-houses of most of the unfortunate persons who, from ill-directed motives of compassion, are carried into these charities. In the two great hospitals of St. Thomas and St. Bartholomew, in London, about six hundred patients die annually, which is about one in thirteen of those who are admitted as patients.

"In Paris it is supposed that one-third of all who die there die in hospitals. The Hôtel Dieu—a vast building situated in the middle of that great city—receives about twenty-two thousand persons annually, one-fifth of which number die every year. It is impossible for a man of any humanity to walk through the long wards of this crowded hospital, without a mixture of horror and commiseration at the sad spectacle of misery which presents itself. The beds are placed in triple rows, with four and six patients in each bed; and I have more than once, in the morning rounds, found the dead lying with the living; for, notwithstanding the great assiduity and tenderness of the nurses, some of whom are women of family and take the veil and piously devote themselves to that office, yet it is almost impossible, from (p. 83) the vast number of patients, to bestow timely assistance upon every individual.

"If we compare the number of patients who die in the county infirmaries of England with those of the London and Paris hospitals, the proportional difference will be greatly in favor of the former—in the Northampton Infirmary one in nineteen dies annually, and in that of Manchester, placed in a more airy situation, one in twenty-two; and, although the putrid air of great cities is more unfavorable to health in general than that of country towns, yet the great difference in mortality will be found, upon a close and fair examination, to

arise from the structure and crowded wards of the hospitals in overgrown capitals.[1]

"For, if to the comparison between the mortality in large city hospitals and those of country towns we further add the proportional difference between the last and that of private practice, it will be found to be in favor of the latter. From all which facts it evidently appears how essentially necessary pure air is to the cure of diseases in general, and particularly those which arise from putrescent causes, either internal or external.

"It is computed that a gallon of air is consumed every minute by a man in health, and much more must be necessary to one who is sick, as the morbid effluvia which are continually exhaling from all parts of the body and lungs must contaminate a larger portion of the surrounding atmosphere, and render it less healthful to breathe in, for animals are observed to die much sooner in foul air than *in vacuo*.

"But, the preceding facts not having been sufficiently understood or attended to, a false economy has universally prevailed in the structure of hospitals for the sick; for those that have hitherto had a principal direction, both in the architecture and management of them, have confined their views entirely to objects of conveniency, cheapness, or ornament; and, in one of the last hospitals built in London for lying-in women, there is more expense bestowed on an elegant chapel in it than would have furnished four wards.

"In short, the physician and architect have, generally, two very opposite and incompatible views—the latter laying out his plans so as to contain the greatest number of persons in the least possible space, whereas the former always aims at

[1] "It is to be hoped that the hospital lately built in this city will have fewer objections to its plan than any hospital hitherto constructed; the principal wards—which are to contain no more than eight beds—are thirty-six feet in length, twenty-four wide, and eighteen high; they are all well ventilated, not only from the opposite disposition of the windows, but proper openings in the side-walls, and the doors open into a long passage or gallery thoroughly ventilated from north to south."

having the utmost room which is consistent with use and conveniency.

"The same false maxims of economy which have prevailed in the construction of hospitals in large cities are too much adopted in the military hospitals of camps and garrisons, as evidently appears from the complaints made of them by Sir John Pringle, to whose excellent observations on the diseases of the army I am principally indebted for the following remarks on the means of preventing diseases in camp or garrison."

These pages, written one hundred and one years ago, in their comprehensiveness reach down to this very day; they show that Dr. Jones had studied the subject, and was well prepared to take advantage of his trip abroad, to see understandingly the hospitals as they were, and to imbibe fully the spirit of hospital-reform at that time at work in England, and soon to express itself in France through the famous report of the Academy of Sciences.

Dr. Jones's account gives us a very good idea of the construction and sanitary condition of hospitals, just previous to the Revolutionary War. All writers of that period substantiate the statements made by Dr. Jones that the condition of the hospitals in France was very bad; but, if the death-rate in the London hospitals was only one in thirteen, their hygienic condition must have been as good as it is to-day. It is interesting and curious to compare this most excellent plan given by Dr. Jones as that of the New York Hospital, destroyed by fire more than one hundred years ago, with the *seven*-story building, just erected for the New York Hospital, on a space of ground only seventy feet by one hundred and seventy-five in extent, which is to accommodate one hundred and fifty patients. The contract for the building alone is $432,000—one would imagine that the century had been reversed.

In the very beginning of the Revolutionary War great solicitude was shown for the proper care of the sick and wounded soldiers. After the battle of Breed's (or Bunker) Hill, a hospital was established at Cambridge "in several pri-

vate but commodious houses," and Dr. John Warren, a brother and pupil of Dr. Joseph Warren, who fell while commanding the troops in that battle, was placed in charge, and soon after this several hospitals were established around Boston.[1]

General Washington, after his first inspection of the army as commander-in-chief, on the 21st of July, addressed the following letter to the President of Congress: "I have made inquiry into the establishment of the hospital, and find it in a very unsettled condition. There is no principal director, nor any subordination among the surgeons; of consequence, disputes have arisen, and must continue until it is reduced to some system. I could wish it was immediately taken into consideration, as the lives and health of both officers and men so much depend on due regulation of this department."

Notwithstanding this kind consideration evinced for the sick and wounded by the commander-in-chief, and by the whole American people during the Revolution, there was much suffering on account of the poverty of the Government and the meagre resources of the country, preventing the possibility of building and conducting hospitals, consequently it was a necessity to make use of all kinds of houses for the purpose of treating the sick and wounded; and we find but little to record as adding to the development of hospitals. The first director-generals of the hospitals during the Revolution, as Dr. John Morgan, Dr. William Shippen, and most of the older and controlling surgeons, favored general hospitals, but there were several advocates of small regimental hospitals after the teachings of Sir John Pringle, among them Dr. Benjamin Rush and Dr. James Tilton.

Dr. Rush, in his "*Medical Inquiries and Observations*," speaking of the hospitals of the Revolution, says: "Hospitals are the sinks of human life in an army. They robbed the United States of more citizens than the sword.

"Humanity, economy, and philosophy, all concur in giv-

[1] Brown's History of the Medical Army Service of the United States.

ing a preference to the convenience and wholesome air of private houses; and, should war continue to be the absurd and un-Christian mode of deciding national disputes, it is to be hoped that the progress of science will be so great as to prevent, so far, one of its greatest calamities, and to produce an abolition of hospitals for acute diseases.

"Perhaps there are no cases of sickness in which reason and religion do not forbid the seclusion of our fellow-creatures from the offices of humanity in private families, except where they labor under the calamities of madness and the venereal disease, or where they are the subjects of some of the operations of surgery."

In the beginning of the War of 1812, Dr. James Tilton, of Delaware, published a small volume of "*Economical Observations on Military Hospitals and the Prevention and Cure of Diseases incident to an Army.*" In this he gives his experience during the Revolution, and describes the plan of hospital-huts and organization, presented by him to Congress in 1781; on page 13 he says: "It would be shocking to humanity to relate the history of our general hospitals in the years 1777 and 1779, when it swallowed up at least one-half of our army, owing to a fatal tendency in the system to throw all the sick of the army into the general hospitals; whence crowds, infection, and consequent mortality too affecting to mention." Again he says: "My brethren of the faculty will probably think it an interesting fact that more surgeons died in the American service in proportion to their number, than officers of the line; a strong evidence this, that infection is more dangerous, in military life, than the weapons of war." Page 47, he adds: "The cardinal point or principle to be observed in the direction of all hospitals is to avoid infection; when this can be done, the practice of hospitals differs little or nothing from private practice. But, where infection or foul air is suffered to prevail, no skill or address in practice can much avail. The cause must be removed before the patient can be relieved by medicine."

Dr. Tilton speaks favorably of tents for use as hospitals in

warm weather, but adds: "In cold climates and winter seasons some better protection than tents afford may be necessary. In such cases the best hospital I have ever contrived was upon the plan of an Indian hut. The fire was built in the midst of the ward, without any chimney, and the smoke, circulating round about, passed off through an opening about four inches wide in the *ridge* of the roof, etc. This was the expedient I employed in the hard winter of 1779–'80 when the army was hutted near Morristown, and I was well satisfied with the experiment." He gives a ground-plan and elevation for the log-hut hospital.

Dr. Tilton was appointed Surgeon-General of the United States Army in 1813, when the office was created, and he succeeded in conducting the hospitals of that war very satisfactorily. Besides the works of Drs. Rush and Tilton, describing the American hospitals during the Revolution on the American side, and Dr. Jackson those of the British, we find little of much note.

About the time of the Revolutionary War in America, the most important movement, certainly the one that eventually had the greatest influence in bringing about a reform in the construction of civil hospitals, was made in France just after the great fire of 1772, when a part of the Hôtel Dieu was burned. The condition of the hospital at that time was such that many favored its removal. The idea was, to divide its inmates among several hospitals, smaller, and situated farther from the centre of the city. The agitation of the subject resulted in a committee being appointed from the Academy of Sciences to report upon a plan. The committee was composed of the most eminent men of that period, Ténon, Bailly, Lavoisier, Laplace, and others, who in their report of the 22d of November, 1786, to the ministry of Louis XVI., urged the removal of the hospital. At that time the inmates numbered five thousand, although the number of beds was only about two thousand, all the beds being double, and many holding four or six patients at one time.

The committee proposed to replace it by four hospitals of

twelve hundred beds each. They claimed that "a sick ward should be entirely detached from other buildings, so that its walls may be constantly exposed to sun and wind, and that draughts of fresh air may constantly renew an atmosphere which is perpetually fouling itself." They preferred that these hospitals should have only one floor of wards, but, as this required too much ground, the plan was modified. A Government edict, June, 1787, ordered the establishment of four hospitals at the four cardinal points of Paris, for twelve hundred beds each, and after the plans suggested by the committee of the Academy. The political events which followed prevented the execution of the project.

A supplementary report, dated the 12th of March, 1788, lays down more precisely the principles which the plan of a model hospital resolved on by the committee represents.

They proposed, as before, detached pavilions arranged in parallel lines; that the buildings for the offices, kitchens, pharmacy, and other administrative purposes, should be in in front; that the pavilions on one side should be for men, and those on the other side for women, with the chapel and operating-room, etc., in the rear of the plot.

They proposed that the pavilions should be three stories in height, twenty-four feet wide by one hundred and sixty-eight feet long, the ends of the buildings for thirty feet being wider, and containing the service-room for the wards; the ward to be one hundred and eight feet long and fourteen or fifteen high. They proposed that each ward should contain thirty-four to thirty-six beds in two rows, each ward having its own English water-closets, lavatory, kitchen for special diet, and sister's or nurse's room with every thing at hand for the care and comfort of the patients.

They proposed that each pavilion should be separated from the next by a garden about seventy-two feet wide and as long as the building, with nothing in it to intercept the air, and to serve as airing-grounds for the patients of each pavilion. They proposed that the pavilions should be connected by a corridor running round the whole of the central court, past

the foot of the stairs in each pavilion. It was not to rise above the ground-floor, so as not to intercept the circulation of air.

A part of the committee visited England, and along with the idea of the English water-closets, baths, etc., they were impressed with the necessity of limiting the beds in a ward to from twelve to thirty, a custom entirely at variance with that which prevailed in the Hôtel Dieu, where double-beds at that time were multiplied in a ward to the number of two or three hundred. Said the Academy committee: "It is a mistake to suppose that a partition-wall will divide a ward of fifty beds into two wards of twenty-five beds each. Contiguous, communicating wards are in reality but one ward, and have a common atmosphere."

Husson, from whose works the above was taken, says that "the wards built for La Pitié, in Paris, 1792–1802, as dormitories for the orphans of soldiers, for whose use the building was taken, were the first instance of the application of the principles recommended by the Academy of Science." France was convulsed with revolutions, and the practical results of the committee's work followed but slowly this small beginning.

In 1788, by royal order, the memoirs of Ténon were published, and added a valuable work to hospital literature.

The work of John Howard, the philanthropist, from 1789 to 1793, in visiting all the prisons, lazarettos, and hospitals, of England and Europe, by drawing public attention to the subject, caused not only reform in the sanitary condition and management of prisons, but of hospitals also. He recommends that hospitals should be placed out of town, and consist of buildings not more than two stories high, with large and opposite windows, and above all that in a hospital there should be perfect cleanliness.

Soon after Howard, appeared the philosophical essays of Count Rumford, who devoted his life and a large fortune to the purpose of improving the condition of the poor. He endeavored to teach the laboring classes the practical application of

hygiene in their houses, and to him is due the credit of making the first effort to systematize and direct charitable relief, so that it might not do more harm than good. Such a work as this must have influenced the internal arrangements and management of hospitals.

In 1803, soon after the state assumed control of the hospitals in France, the report of the Council General did much toward improving the management of the hospitals of Paris. Reform in classification, cleanliness, nursing, and change in the management in general, were recommended.

During the first half of the present century many hospitals were built, and several well-known works, that had some influence on hospitals, were published; among these are the writings of Morneau, in 1802; Pastoret, in 1808; Cutbush, in 1808; Clavereau, in 1810; Larrey, in 1812; Barton, in 1814; Hennan, in 1820, etc., etc.; and there was a gradual improvement made in the internal arrangements; but, except in matters of this kind, nothing was advanced that could be considered an improvement upon the principles and plans given in the report of the committee of the French Academy of Sciences.

In fact, with one or two exceptions, in none of the many hospitals established between 1786—the date of the report—and 1860 were the advanced views of the committee carried out. Almost all the hospitals built before 1860 are large, solid, many-storied structures, with the wards and administrative offices, etc., all in one building.

In 1829 a hospital was built at Bordeaux reproducing the plan recommended by the committee; and it was not until 1854, fifty-eight years after the report, when the Lariboisière was finished, that Paris had a hospital to show as the fruits of it. The plan of the Lariboisière is almost exactly that proposed by the committee, and is only an improvement on it in detail, excepting, perhaps, the ventilating apparatus.

Previous to 1859 neither England nor the United States had a civil hospital that would compare favorably with the plan proposed by Dr. John Jones in 1773. One of the first

pavilion-hospitals built in England, after the plan proposed by the French Academy's committee, was Blackburn Infirmary, in 1859. The first in the United States was the Episcopal Hospital of Philadelphia, founded in 1860.

In Germany there seems to have been but little progress in the development of hospitals until the early part of this century, when the plan of hospitals known as the corridor-hospital was introduced and very generally adopted; nor did the Germans improve upon this very objectionable plan until within the past ten years.

The hospitals of Russia were modeled after those of Germany, and were in no respect better.

The next progressive step, after the report of the French Academy's committee, in the development of hospitals was brought about by the fearful death-rate of the English and French armies in the beginning of the Crimean War. So great was the mortality, that the whole English people were aroused to the necessity of better provision being made for the sick and wounded. Miss Nightingale, who had the training as a nurse at Kaiserwerth, selected a band of thirty-seven nurses, and left for the seat of war on the 24th of October, 1854, and in 1855 the Government appointed a sanitary commission to proceed at once to the seat of war; the members of this commission were John Sutherland, M. D., Hector G. Milroy, M.D., and Robert Rawlinson, Esq., C.E. They succeeded in introducing many valuable sanitary reforms.

In 1854 Michel Lévy, Sanitary Inspector of the French Army in the Crimea, suggested the use of wooden barracks or huts for hospitals, and at the same time proposed a permanent tent-hospital. It was found by experience that simple wooden huts, raised from the ground, with double walls—to protect from the heat in summer and cold in winter—with ridge ventilation, and heated by means of open fires or stoves, gave far better results than any other kind of building.[1]

The following, afterward expressed by Dr. Sutherland,

[1] Report of the Proceedings of the Sanitary Commission dispatched to the Seat of War in the East, 1855–1856.

will give the best idea of what was taught by the experience of the Crimean War. He says: "But, while admitting that large buildings may be improved if there be time for doing so, it must be stated that no more disastrous idea can take possession of men's minds than that sick and maimed people ought, on grounds of humanity, to be packed into churches, barracks, and other unprepared buildings, with as little delay as possible. This error has slain its tens of thousands in all wars."

The indefatigable and practical work of Miss Nightingale in the hospitals during the Crimean War, and her masterly-written answers to the questions of the "Commissioners appointed to inquire into the Regulations affecting the Sanitary Condition of the Army and the Organization of Military Hospitals, and the Treatment of the Sick and Wounded," published in 1858, have justly made the name of Florence Nightingale the most famous not only in connection with nursing, but hospital-construction also. In 1859 the first edition of her well-known book, "*Notes on Hospitals,*" appeared.

This book of Miss Nightingale, on account of its real worth and the earnest, practical, and telling way in which it is written, has done more to bring about reform in hospital-construction than any other work ever written. And her book, "*Notes on Nursing,*" has made her name a household word wherever the English language is spoken.

These two books of Miss Nightingale were the first to reach and educate the *people.* The general interest in hospital-reform was very great in England for years after the Crimean War, and the discussion of the subject between the advocates for new and improved hospitals to replace the venerable old hospitals, and the non-progressive and conservative party, still shows itself. The "General Commission appointed for improving the Sanitary Condition of Barracks and Hospitals" made a report, April, 1861, signed by John Sutherland, W. H. Burrell, and Douglas Galton, which accepted the experience of the Crimea, and the suggestions of Miss Nightingale, and settled the question in regard to military hospitals by the adoption of what is known as the barrack-hospital.

In answer to the reforms proposed by Miss Nightingale, and to counteract the general feeling that all old hospitals should be replaced with new ones, the medical officers of the Privy Council, Dr. Bristowe and Dr. Holmes, after visiting almost every hospital in the United Kingdom, made a most excellent but *very conservative* report.[1]

Sir James Y. Simpson took up the subject and brought forward statistics to show the relative mortality of private practice and small hospitals in the country, compared with the mortality of large city hospitals, and, in his paper on "Hospitalism," advocates small cottage-hospitals, outside the city limits. He was answered by Matthew Duncan on the conservative side.

Lately the subject of hospitalism was warmly discussed in the British Medical Association, and in 1874 Mr. Erichsen published, in book-form, a course of lectures on "Hospitalism." This little book, in which the need of hospital-reform is plainly shown, not alone in the matter of construction, but especially in all that concerns internal management, is of very great value.

The practical results of the interest in hospitals brought about by the Crimean War were, the building of the famous Herbert Hospital at Woolwich, and establishing on a sure basis the detached pavilion-plan of hospital-construction, which had been proposed, and a small one erected (Plymouth Naval Hospital), just one hundred years before. The Herbert Hospital is an improvement upon the Lariboisière; but, like it, is modeled after the plan proposed by the French Academy's committee of 1786—the improvement being mainly in the details of internal arrangement.

Until the building of the Herbert Hospital, the Lariboisière was the model hospital of the world. Now, in the United Kingdom, the Herbert Hospital is the model; and we know of none in Great Britain that can be called an im-

[1] Sixth Report of Medical Officers of the Privy Council, Eyre & Spottiswoode, London, 1863.

provement upon it, except in the amount of cubic air-space allowed each bed.

Since the building of the Herbert Hospital, several hospitals have been erected; among these the new St. Thomas's, which only surpasses the Herbert in magnificence of construction; but, instead of two floors of wards to the pavilion, has three with an attic, and all the pavilions on one side of the corridor.

The Rotherham Hospital, lately finished (1874), has three small one-story wards. It is the only civil hospital built on the one-story plan that we know of in England.

The influence of the Crimean experience did not do so much for France. Mr. Husson's well-known work was published in 1862, but nothing better than the Lariboisière was proposed. In Germany the corridor-hospital had not been improved upon, nor until within the last ten years had any important progress been made.

Before the interest in sanitary and hospital reform caused by the Crimean War had quieted down, the American people had an opportunity afforded them to make use of the valuable suggestions published in the reports of the English commission, and, in doing so, succeeded in developing the most perfect system of army-hospitals ever known to the world.

Early in the late civil war a sanitary commission was organized, of which the Rev. H. W. Bellows was chairman.

In July, 1861, the committee on hospitals of this commission reported: "Your committee venture to embody their conclusions in form of suggestions, and would submit to the commission the propriety of recommending to the Government that hereafter, instead of hiring old buildings for general hospitals, they should order the erection of a sufficient number of wooden shanties or pavilions of appropriate construction, and fully provided with water for bathing, washing, and water-closets, and ample arrangements for ventilation and for securing warmth in winter, to accommodate from thirty to sixty each, and to be sufficiently separated so as not to poison each other. This suggestion embodies the latest and best views as to the

construction of hospitals, and its adoption would save both time and money." And at the close of the October session of the commission it was understood that the Government would at once commence the erection of two cheap temporary model hospitals at Washington, "in conformity with plans carefully prepared by a committee of the medical members of the commission, and approved by it, as embodying the latest results of sanitary science. The plans have been formally approved by the quartermaster-general, the commander-in-chief, and the medical director of the Army of the Potomac, and the ground for the example-building has been staked out." [1]

The medical members of the commission were William H. Van Buren, M. D.; Wolcott Gibbs, M. D.; Robert C. Wood, M. D.; Samuel G. Howe, M. D.; Elisha Harris, M. D.; C. R. Agnew, M. D.; J. S. Newberry, M. D. Mr. F. L. Olmsted, civil engineer, was also a member of the commission.

The works of Miss Nightingale and the Crimean experience are frequently quoted by the committee, and the plan of army-hospitals adopted was the practical application of the temporary one-story Crimean hut with ridge ventilation extended to the size of the wards recommended by Miss Nightingale. The wards were connected by a corridor, and the first ones erected had the pavilions arranged on opposite sides of the corridor after the plan of the Blackburn Infirmary.

The position of the windows, the water-closets, nurses' rooms, etc., were usually placed at one end of the ward, or divided between the two, the water-closets being at the free extremity, and the nurse's room and dining-room next to the corridor. The number of beds in each ward varied from twenty-five to fifty.

The wards were temporary in character, with ridge ventilation, and usually heated by stoves.

The administrative buildings were separate from the pavilions and connected with them by the corridors; the wards

[1] United States Sanitary Commission Work and Purposes, New York, 1864.

were always one-story buildings. In fact, the plan really differed from that recommended by Miss Nightingale in these respects: the pavilions were only one story high, and were temporary in character, and always had ridge ventilation. The first hospitals were for only two hundred and fifty beds, but later they were much larger, one, the "West Philadelphia Hospital," contained 3,124 beds in wards of forty-eight beds each.

The system was thoroughly tested during the war on a scale never before equaled, there being at one time in the Government hospitals as many as 134,000 beds. The success was such that an army averaging 744,346 men passed through a four years' war with an annual death-rate of only eighty-eight per thousand from all causes, and it is estimated that thirty-three out of eighty-eight were violent deaths, leaving fifty-five from disease in one thousand.

The same system and plan of hospitals were adopted by the Confederate army during the war.

The experience of the war may be said to have developed and established the following principles in the construction of hospitals:

1. That the hospital should be placed on a large area of ground, so that the pavilions can be widely separated from the administrative buildings and from one another.

2. That the wards should be *only one story* in height, and be ventilated by openings along the ridge of the roof.

3. That the ward-pavilions should be put up not to remain for generations to come, but only so long as they are free from infection, and that, when once they are infected, they should be destroyed, and replaced with entirely new structures.

Soon after the war, one-story wood pavilions of a temporary character were put up on Blackwell's Island, New York, for the treatment of contagious fevers, and in different places in the United States. Small ones were built in connection with large hospitals for infectious cases; but the first complete civil hospital constructed after plans used during our Amer-

ican war, is to be found in Germany. In 1867–'68 a one-story ward-pavilion hospital was planned and soon after built in Leipsic. The old stone hospital is used as an administrative building. There are fourteen one-story ward-pavilions, or shed-hospitals as the Germans call them. They are frame buildings filled in with brick, one hundred feet long, thirty-two feet wide, fifteen feet high at the eaves and twenty at the ridge. They are placed sixty feet apart, raised four feet from the ground on stone piers, and are connected by a corridor. They have ridge ventilation.

Each ward or shed has twenty-four beds, with nurse's room, baths, kitchen, and closets, complete. During twelve months, Prof. Thiersch, who has charge of the surgical clinic, performed two hundred and sixty-six serious surgical operations, and did not lose a case from pyæmia; while, prior to the construction of the new pavilions, in the old stone hospital, which is now the central building, he lost from forty to fifty amputations from this cause annually.

In 1867, at Berlin, a one-story ward was built in connection with Charity Hospital by Dr. Esse. Kiel has two stone one-story ward-pavilions used as a garrison hospital, and at Dresden one-story ward-pavilions have been built.

Dr. Stephen Smith, of New York, wrote an essay upon hospital-construction, and recommended a plan for the Roosevelt Hospital. The plan recommended by Dr. Smith in this essay is very much the same as that of Miss Nightingale. The Roosevelt Hospital has one one-story pavilion to show, as the influence of the experiences of the war, but New York City has had five other large new hospitals, all of which are massive, many-storied buildings, costing immense sums of money; and one of these, the last, is seven stories, and on a space of ground seventy-five by one hundred and seventy-five feet.

In Boston, Massachusetts, the one-story pavilion plan with ridge ventilation has been adopted, and there are two or three pavilions erected on the grounds of the Massachusetts General, and one on the ground of the Boston City Hospital, which

surpass any in this country, except perhaps one lately constructed in Philadelphia.

The organization of our Sanitary Commission and the plans for military hospitals were carefully studied and efficiently used by the Germans in the late Franco-German War.

So great was the success in treating the wounded in the hospitals constructed on the American plan, that since the war the Germans, in their thorough and exhaustive way, have taken up the subject with the view of applying these principles to their civil hospitals. Some of the first men in medicine and science have written monographs on the subject, such as Virchow, Steinberg, Esse, Leisrink, etc., and some of the professors of hygiene and surgery take up hospital-construction as a part of their course in lecturing to students.

Within the past ten years several books and many monographs and papers have been published on the subject of hospitals, which we have not mentioned. Among the books are: "*On the Construction of Hospitals*," by Douglas Galton, 1869; "*Handy Book on Cottage-Hospitals*," by Horace Swete, 1870; "*Notes on Lying-In Institutions*," by Florence Nightingale, 1871; and "*Hospital Construction and Organization*," Johns Hopkins Hospital, 1875. Through the munificence of the late Johns Hopkins, the trustees of the Johns Hopkins Hospital, of Baltimore, have at their command three millions of dollars ($3,000,000), and fourteen (14) acres of land to establish a General Hospital of four hundred (400) beds. We trust they will build a model hospital embodying all the late improvements, which will be worthy of the Centennial Year of our Republic.

DIPHTHERIA AND ITS TREATMENT, WITH STATISTICS OF ONE HUNDRED AND SEVENTY-NINE CASES.

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Read March 16, 1876.

In December, 1753, there was read to a Society of Physicians in London an "Extract of a Letter from Cadwallader Colden, Esq.," a retired eminent physician, of this city, "to Dr. Fothergill, concerning the Throat Distemper," dated "Coldenham, N. Y., October 1, 1753." An original published copy of the work containing this treasure, for such it is from its intrinsic merits, as well as from its antiquity and its local associations, is now in the library of our President, where, through his courtesy, I have seen it.

In the library of this Academy, contributed to it by Dr. Purple, is a very rare and valuable pamphlet, entitled "An Inquiry into the Nature, Cause, and Cure of the Angina Sufocativa, or Sore-Throat Distemper, by Samuel Bard, M. D., Professor of Medicine in King's College, etc., etc. New York, 1771. 8vo, pp. 33." This was translated into French by M. Ruette, and published in Paris in 1810. This was eleven years before the publication of Bretonneau's first paper.

From those early beginnings down to this Centennial year the medical profession of this city have continued to make, from time to time, valuable additions to the sum total of known facts relating to this fearfully interesting disease.

Following, though at a distance, such worthy examples, I shall now have the honor to present a few general remarks based on the personal observation of more than three hun-

dred cases of diphtheria, and an account of my treatment, with its results, in one hundred and fifty-seven cases.

For more than three years diphtheria has been continuously and increasingly epidemic in this city. In 1873, four hundred and eighty-two deaths were reported from this cause; in 1874, sixteen hundred and sixty-five; in 1875, twenty-three hundred and twenty-nine. Until radical sanitary reforms shall have been effected, it is likely to continue to tax heavily our therapeutical resources.

Great diversity prevails in the views held as to the nature of this disease, and still greater in the methods employed in its treatment.

Of its nature two opposite views are maintained: one, that it is a constitutional disease, with local manifestations; the other, that it is a local disease, with constitutional effects. Mild cases suggest the latter view; severe ones *seem* to uphold the former. In many mild cases we see simply a local inflammation, accompanied with membranous exudation, of moderate intensity, tending to recovery, and throughout its course as free from any accompanying indications of blood-infection as is simple tonsillitis. In severe cases we see overwhelming constitutional disease; but does this necessarily imply a specific primary blood-poison? The result of my own observations has been to make me a convert to the minority who believe that it does not, and that the source of the constitutional disease is in all cases to be found in the local affection. I have been led to this belief mainly by the three following facts:

1. In the great majority of the constitutional cases which I have seen, the local affection has been much more severe and extensive than in the other class. In this remark I am corroborated by writers of various pathological views. Dr. J. Lewis Smith, for instance, says, in his work on the "*Diseases of Children:*" "Whatever may be our opinion on the nature and causes of diphtheria, clinical observations show that the gravity of the malady is, in most instances, proportionate to its local manifestations, at least in the commencement of the

disease. If, by our treatment, we can limit the exudation to a small surface, or can remove it, so that the inflammation, from croupous, becomes catarrhal, at an early stage of the malady, the patient is probably safe." [1]

2. The constitutional disease as I have seen it has been, in order of time, not antecedent to, but consequent upon the local affection. In many cases, it is true, fever has been the first symptom to attract attention; in a few, convulsions have preceded the formation of membrane; but in every case of uncomplicated diphtheria which I have examined at this stage, and the number has not been small, I have found inflammation of the throat present. In numerous instances in which considerable membranous exudation already existed there had been no manifestation of pain or soreness in the throat. This insidiousness of the invasion of the disease has doubtless been a cause of much error as to the order in which its symptoms have actually occurred. It certainly was so in my own case, until experience taught me to examine the throat of every child whose illness was not otherwise plainly accounted for. So unvarying have been the results of my observations since, that I am confident that the more uniformly this rule is put in practice the fewer will be the cases of diphtheria in which the constitutional symptoms will be supposed to have preceded the local ones.

Another common source of error in regard to this point is the fact that diphtheria *often supervenes upon some other disease.* A child, for instance, has taken cold, from the effects of which he is drooping and feverish for several days. Upon the catarrhal sore-throat thus produced diphtheria sets in. The previous symptoms are naturally supposed to have been those of the invasion of the latter disease, when in fact they were nothing of the kind. This mode of origin is very common. Diphtheria also supervenes on aphthæ, on herpetic and common ulcerative tonsillitis, on the sore-throat of scarlatina, of measles, and of influenza, on enlarged relaxed tonsils, etc.,

[1] This is exactly what my treatment accomplishes in the great majority of cases.

etc. No wonder that the minds and the descriptions of those who regard it as a primarily constitutional disease have been hopelessly confused among its various modes of origin. Regarding it as a local disease developing on a soil prepared for it by various prior affections, this confusion vanishes, and the whole subject becomes simple and comprehensible.

3. The results of treatment on the principle of local disinfection, reported from many reliable sources, and embracing a great number of cases, strongly confirm this view. To make a small addition to this mass of facts is the principal object of this paper. Some eminent authorities who hold to the constitutional theory of the disease, unqualifiedly corroborate this testimony. West, for instance, who teaches that diphtheria is a constitutional disease like scarlatina or measles, makes this significant admission: "There is, however, usually a very marked connection between the early arrest of the deposit, however effected, and the speedy recovery of the patient." After quoting from Trousseau to the same purport, he continues, "Local remedies take a very foremost place in the treatment of diphtheria." By what logical relation he could connect his therapeutical facts and his pathological theory, I am at a loss to conceive.

The great majority of the cases of which I shall presently speak, exhibited, under local disinfectant treatment, just so much appearance of general disease[1] as would be produced by the same amount of simple sore-throat, and no more. A very intelligent physician, who has recently watched a number of these cases with me, has told me that, although previously a firm believer in the constitutional theory, so strongly has he been impressed by this circumstance, that he has adopted the opposite view.

These three concurring clinical facts have constituted to

[1] It *should* be unnecessary to point out the distinction between the symptomatic disturbances, often very severe, which may attend any inflammatory attack, and symptoms which indicate constitutional infection, or blood-poisoning. Yet these are often strangely confounded in the arguments of some advocates of the constitutional origin of diphtheria.

my mind strong presumptive evidence that the primary disease and the source of all constitutional infection in diphtheria is the local affection.[1] Rare cases are indeed said to occur that seem difficult of explanation by this hypothesis; but these exceptions, though they certainly do not "prove the rule," are too few to weigh heavily against it. Is it only in diphtheria that we sometimes see rapid and intense septic poisoning from an apparently very inadequate source?

Dr. Samuel Bard wrote as follows in 1771: "This disease I have described appeared evidently to be of an infectious nature. All infection must be owing to something received into the body. This, whatever it is, being drawn in by the breath of a healthy child, irritates the glands of the fauces and trachea, as it passes by them, and brings about a change in their secretions."

Prof. Jacobi, whose contributions to the literature of diphtheria bear the impress of extensive and philosophical study of the disease itself, and, to speak moderately, are second in value to none, sums up what I believe to be the true pathology and treatment of diphtheria—harmonious like all parts of truth, the one with the other—in the following well-chosen language:

"Although finally a constitutional disease, it is at the beginning nearly always local, or, in other words, infection enters the blood at a limited portal, which is the same in the great majority of cases. From this point of view, diphtheria is analogous to the septicæmia of wounded men and of puerperal women, and the local disinfection, which has been accepted as the sheet-anchor in the treatment of these affections, must be also the main reliance in that under consideration. We may congratulate ourselves upon this fact; since we do possess some positive knowledge in regard to the disinfection of accessible putrid fluids, while it is safe to say that as yet we have no proof of our ability to disinfect the blood of the living body."[2]

[1] In taking this view, I do not adopt, nor take at all into consideration, the bacterial theory of Oertel and others.

[2] "Contributions to the Pathology and Therapeutics of Diphtheria," in *The American Journal of Obstetrics*, February, 1875.—There is, in my

I have dwelt thus long upon the question of the primary nature of diphtheria, believing it to be no trivial topic of casual discussion, but a problem the earliest possible right solution of which by systematic clinical and pathological observation and experiment is of the utmost importance, in view of its direct and vital relation to the treatment of a disease which is annually desolating so many homes.

Regarded as a primarily local disease, diphtheria presents three essential elements: first, the contagium, respecting which I know nothing; second, the inflammation, with its destruction of epithelium and its pathognomonic exudation, its accompanying nerve-irritation and symptomatic fever; third, the resulting specific and septic poisons. The indications of treatment are, of course, first, to destroy the contagium; second, to subdue the inflammation. This we shall accomplish, if at all, mainly by removing its cause. Third, to combat the absorption of poison. This we shall do with more or less success by washing away and disinfecting the offensive secretions from the affected surfaces, and by the employment of remedies which locally contract the absorbent vessels—failing in which, constitutional remedies will avail us but little in bad cases. In short, the treatment of diphtheria is local disinfection.

This achievement, so simple in the announcement, presents many practical difficulties, which are, indeed, in some cases quite insurmountable. The problem is to apply to a highly sensitive inflamed surface agents energetic enough to destroy morbific germs, and neutralize putrescent poisons, and yet mild enough not to increase by irritation the inflammation that we would subdue. They must also, in many cases, be conveyed to the inaccessible recesses about the pharynx and

opinion, more essential and valuable truth respecting this disease in this little monograph than can easily be found elsewhere. It should be carefully perused by all students of this much perplexed subject. It is proper to add that while I coincide with Prof. Jacobi's views in almost every particular, I am not his "follower," except in the order of publication. My own pathological conclusions and my present mode of treatment were independently arrived at (as many of my friends know) before his paper was written, or I knew anything of its author's views.

the posterior nares—particularly inaccessible in the case of very young patients, who struggle with wonderful and too often fatal success against the efforts of their would-be preservers. Moreover, in very malignant cases the inflammatory process extends with such rapidity from the surface to the deeper tissues that the sources of blood-contamination are early placed beyond the reach of remedies. Invariable success in the face of such difficulties is doubtless, in the nature of things, impossible. Diphtheria may in time be absolutely erased from the list of death-causes, but it will be by the sanitarian and not by the therapeutist.

Two questions present themselves: 1. What are the best medicinal agents for our purpose? and, 2. What are the best methods of applying them?

The list of disinfectants which have been employed with more or less success in the treatment of diphtheria is very numerous. Among these none has received so general and so authoritative testimonials for practical value as the tincture of the chloride of iron. Some interesting observations on its mode of action are given in Prof. Jacobi's paper from which I have previously quoted. It has seemed to me not only to be a valuable local disinfectant, tonic, and astringent, but also to oppose by some constitutional action the absorption of septic poison.

I think that next to this in order of value stands limewater, which is disinfectant and solvent to diphtheritic membrane, and is mild and harmless.

Third, I should place glycerine, which is disinfectant and solvent, and valuable for its pleasant taste.

Chlorate of potassa, carbolic and salicylic acid, and the sulphite of soda, are septicides too well known to need remark.

The methods of applying remedies in diphtheria are greatly controlled by the circumstance that it is usually a disease of children. Out of more than three hundred cases, I remember only four in adults of mature years. Perhaps a dozen others were in young persons over fourteen years of age. The great majority were in children under twelve. Although most of these little patients were nursed incessantly by their parents

or other relatives, often in the worst hygienic circumstances, in only one case that I knew of was the disease communicated to an adult. Of 124 cases visited in my dispensary district in 1875 the ages were as follows: 8 of one year (that is, between one and two), 12 of two, 16 of three, 13 of four, 11 of five, 15 of six, 11 of seven, 12 of eight, 7 of nine, 5 of ten, 1 of eleven, 4 of twelve, 3 of thirteen, 1 of fourteen, 1 of fifteen, 1 of sixteen, 1 of eighteen, 1 of nineteen, 1 of thirty-seven. Of the 4,476 deaths from diphtheria in this city in the last three years, 2,916, or 65 per cent., were of children under five. 3,979, or 89 per cent., were of children under ten. Some authorities speak of diphtheria in adults as if it were common; and some irregular practitioners are said to have treated, and of course cured, great numbers of such cases. Their experience has been peculiar.

Being a disease of children, being comparatively short in its duration, and requiring in its treatment no special manipulative skill, diphtheria, unlike most other throat-diseases, falls mainly under the observation of the general practitioner, rather than of the specialist.

Foremost among the methods of throat-disinfection is the internal administration of medicines. I have employed it in every case. I believe it to be greatly preferable to topical application by brush or probang, which is apt to be irritating and unsatisfactory. It is important that medicines thus administered should be agreeable to the taste, especially in the treatment of young children, and also, of course, that they should be harmless when received in considerable quantities in the stomach. As they are given mainly for their action as throat-washes, the necessity of their frequent administration is evident. I present three formulæ of combinations which I have employed in many cases, and which I recommend as simple, pleasant, effective, and innocuous:

℞.	Tinct. ferri chloridi,	fl. ʒ jss.
	Glycerinæ,	
	Aquæ,	āā fl. ℥ j.

M. S. A teaspoonful every hour.

For children under three years, only one drachm of the tincture of iron in this mixture is sufficient. This combination tastes pleasantly. I have hardly ever known children to object to taking it. Nor have I ever perceived any harm to result from its administration. Where vomiting is present, however, it must sometimes be omitted. Larger or more frequent doses have sometimes seemed to be beneficial in bad cases. I formerly generally accompanied this prescription with the following:

℞.	Potassæ chloratis,	ʒ ss—ʒ j.
	Glycerinæ,	fl. ℥ ss.
	Aquæ calcis,	fl. ℥ ijss.

M. S. A teaspoonful every hour.

This should be given alternately with the preceding one, with half-hour intervals between the two. This frequency of administration should be insisted on, except during the night, when the patient should be allowed to sleep an hour or two at a time if he will.

Instead of this latter prescription, I have in many of my more recent cases employed the following:

℞.	Acidi salicylici,	gr. x—℈j.
	Sodæ sulphitis,	ʒ ss—ʒ j.
	Glycerinæ,	fl. ℥ ss.
	Aquæ,	fl. ℥ ijss.

M. S. A teaspoonful every hour, alternating, at half-hour intervals, with the iron mixture.

The sulphite of soda is used to dissolve the salicylic acid. Theoretically the combination of two such septicides should be especially effective, and so it has seemed to be.

To these I have, when practicable, superadded the following:

℞.	Acidi carbolici,	min. xv.
	Aquæ calcis,	fl. ℥ vj.

M. S. To be applied to the throat very frequently in the form of spray.

It is important to use the right instrument for this purpose. This is a little perfumery-atomizer in which the rubber bulb is attached to the bottle at right angles without

the intervention of a flexible tube. It is "Apparatus No. 56" of Codman & Shurtleff's manufacture. Its great advantage is that it can be used with one hand, in any position. The patient being told to open the mouth widely, the spray should be thrown directly into the throat, the atomizer being held a few inches from the mouth. This should be done for several minutes at a time. To avoid disturbing the patient too frequently, I generally direct it to be done just before or after the administration of each dose of the medicine—that is, every half-hour. I have found this remedy, when faithfully and efficiently applied, of very great value. Its effect is more generally diffused and continuous than that of medicines that are swallowed, and is manifest in accelerating the softening and disappearance of membrane, and deodorizing the breath. It is pleasant to the patient, and can be freely applied to many children who are too young to use gargles. Unfortunately, children under two or three years of age resist its use, which precludes its effective employment in their cases.

The early and thorough use of the nasal syringe or douche is often essential to success. I believe that many cases have been lost because this means has been omitted or too long deferred. We should not always wait until the obstructed and snuffling breathing indicates that the posterior nares are already seriously invaded. If, after a faithful use of the applications which I have previously described, an offensive odor of the breath persists, we may infer that there is some lurking-place of the disease which we have failed to reach, and that this measure is indicated. The operation is certainly not pleasant to the patient, but, if properly performed, is not especially formidable. The child should be seated on the lap of one person, who should secure his hands. Another, preferably a man, should stand behind him and support the back of his head (which should be inclined forward) against his breast, holding it firmly with a hand on each side. A third can then easily make the injection into the nostrils. The more strength and decision are manifested, the less is the child likely to struggle. The syringe should not

be too small, and the fluid should be injected with considerable force. It will then make its exit by the other nostril and also by the throat. This should be repeated a number of times at each sitting, until the nasal passages and pharynx are thoroughly cleansed. I have always used a two-ounce hard-rubber ear-syringe. I usually first inject tepid salt-water until the passages are thoroughly cleansed, and then conclude with one or two syringefuls in each nostril of the salicylic-acid mixture already mentioned. I have usually repeated this two or three times a day.

I have sometimes applied to particularly unyielding membranes, occurring in the older class of patients, a mixture of tincture of iron, two parts, and glycerine, one part. This should be touched very carefully to the surface of the membrane with the tip of a camel's-hair pencil once or twice a day. It seems to shrivel up the membrane, and hasten its disintegration. But too great caution cannot be exercised in making strong local applications. They should never be mopped over the inflamed throat. I have again and again seen this proceeding followed by intensification of the inflammation and spread of the membrane, the whole aspect of the case being rapidly changed for the worse. I doubt if I shall ever apply a brush to a child's throat in this disease again.

I believe that quinine is, as a rule, in the case of young children at least, worse than useless. By its detested bitterness it often causes them to dread the approach of the spoon, and to struggle against all medicine and nourishment.

This evil may easily frustrate a treatment the most important element of which is the very frequent administration of medicines. If there be a frequent struggle and crying, not only are nervous irritation, febrile excitement, and exhaustion, greatly promoted, but also, in this struggling and crying, diphtheritic material or irritating medicine *is likely to be drawn into the larynx, thus setting up croup.* I have seen this connection of cause and result very evident in more than one case.

I believe that no mode of treatment will ever be very successful in diphtheria, of which unpleasantness is an element.

That the importance of this point has been in so many instances practically overlooked, doubtless helps to explain why so many intelligent people, who are far from adopting the absurd theories of homœopathy, yet employ it as the less of two evils, especially for their children.

In the later stage of bad cases, when the nervous energies are yielding to the continued strain, I frequently give a grain of quinine, or, in preference, a drachm or less of the compound tincture of cinchona two or three times a day, which quantity I am unfashionable enough to consider sufficient for children in the absence of definite malarial or pyrexial indications, and *less likely to do harm than more.*

I have also employed a single dose of quinine in a very few instances, with apparent benefit, under circumstances similar to the following:

On the morning of April 15, 1875, I was called, in my private practice, to Carrie S., aged nine, it being the third day of her illness, and the second of the existence of membrane. The temperature then was 103°. In the evening I found the temperature 105½°, pulse 156, much nervous disturbance, membranes extending, with a margin of intense inflammation —the whole showing a tendency toward a malignant form of disease. I at once prescribed six grains of quinine, ice and spray to be used freely. Returning in three hours, I found the temperature fallen to 101°. On the following morning the local appearances had greatly improved. The case thenceforth did well.

Very high fever furnishes a soil favorable to the spread and malignant development of diphtheria, as of most other diseases, general or local, and the temperature in severe cases should be carefully watched. High fever at the onset of the disease is common, and generally abates without special treatment. I think quinine at that stage would be inappropriate and injurious. When the fever of onset is excessive, and accompanied with convulsions or other marked indications of nervous disturbance, I have found a single dose of calomel to promptly relieve these symptoms, and modify favorably

the subsequent character of the disease. Among the many venerable errors formulated from scanty or superficial observation, and perpetuated even by such recent writers as Dr. Cohen, is the one that diphtheria will not bear antiphlogistic treatment. Like all other diseases, it will bear best just that treatment which is adapted to the special indications of each case. The great majority of cases, however, require *no medication* except such disinfectant measures as I have previously described.

Stimulants should not be indiscriminately used in diphtheria. I have treated the great majority of cases without them, and believe this to have been an important element of my success. When unnecessarily or excessively employed, they may be very injurious. Cold milk is generally the best diet through the height of the disease. When this, or other nourishment is not taken freely, eggnog containing a very little brandy or wine is often acceptable and very valuable. Fruit or its juices are positively beneficial. Ice should be given freely, when the patient will take it.

The treatment which I have thus described is simply the most efficient system of harmless local disinfection that I have been able to devise. Its essentials are earliness, thoroughness, and *frequency* of application, and the *careful avoidance of irritation*, with, of course, such modification of these means or adoption of others as may be required by special exigencies. When any of these elements are absent, disinfectant treatment can never be fairly tested or judged.

Before presenting the results of my own treatment, I shall venture a remark or two on results in general. Many cases of diphtheria recover in spite of bad treatment. Many others will recover if let alone, or only harmlessly treated. The fatal tendency of the disease differs widely at different times and in different localities. With no other data to guide me than my own observations of the disease, I should estimate that the spontaneous recoveries would be about sixty per cent.,[1] on

[1] This per cent. probably expresses about the *actual present* average rate of mortality from diphtheria, the good results of some modes of treatment

an average, of all cases. I should be glad to learn the views of other observers on this point. It is certainly one of some practical importance. Are we not constantly reading and hearing of the successes of the most diverse and opposite methods of treatment, not only in regular practice, but in that of homœopathists and eclectics? Until Nature shall receive her due share of credit in our cases of recovery, she will doubtless continue to revenge herself upon us by helping to confound therapeutical science and bolster the pretensions of charlatans.

At the other end of the scale, in order of severity, there are doubtless more than five per cent. of all cases that through especial malignancy or various complications would prove fatal under the best treatment. The thirty per cent. or so that come between these extremes are the possible trophies of successful therapeutics. These proportions may vary so widely in small groups of cases, that statistics of treatment can only be valuable when based on a considerable number.

Another point in regard to which a general and definite agreement is very important is this: What is to constitute for statistical purposes the criterion of a case of diphtheria? Are instances of the "catarrhal"[1] form of the disease to be

which are beneficial being just about balanced by the bad results of others that are injurious. Many methods are a *mélange* of the beneficial and the injurious. Could a large number of cases be left to the *vis medicatrix naturæ* with milk-diet and good nursing, the favorable results as compared with those from an equal number under heroic stimulation and medication and energetic topical treatment would probably surprise the advocates of the latter. The experiment would doubtless be (comparatively at least) an eminently safe one *for the former.* I believe also that the results from cases treated solely by harmless "constitutional" medication—the sulphocarbolates, for instance—given at considerable intervals, just about illustrate what Nature can effect in this disease. *Real* homœopathic treatment of course does the same thing.

[1] The term "catarrhal diphtheria" is not properly used by Oertel. All diphtheria probably begins with, or sets in upon, catarrhal inflammation. In the transition stage from that to membranous exudation there are often, as is stated by most writers, and as I have many times seen, on the inflamed mucous membrane, specks, spots, beads, streaks, or small patches

entered with those of the membranous in the enumeration of results, and that without any distinction being made? I am reliably informed that, of the cases reported to the Board of Health, a considerable proportion have been found on inspection not to be diphtheria at all, or at best very questionable. I am happy to add that most of these spurious returns have come from irregular practitioners, and that their patients have all recovered. Yet it is too true that in many statistics which come from very respectable sources, and in regard to which no unworthy motive can for a moment be imagined, there is great lack of definiteness of statement in this respect, to say the least. Every case of diphtheria mentioned in this paper has been a well-marked one, attended with membranous exudation. If diphtheria has any pathognomonic sign it is this, which is indeed implied in the very etymology of the word; and any attempt to ignore it can tend only to hopeless confusion, and the practical worthlessness of statistics.

In the year 1873 I visited in the South District of Demilt Dispensary thirty-eight cases of diphtheria, and in the first nine months of 1874 fifty-one. In the last three months of 1874 I visited in the North District of the same dispensary, bounded by Twenty-fifth and Fortieth Streets, Sixth Avenue and the East River, twenty-nine cases; and in 1875 one hundred and twenty-four—a total in the three years of two

of a whitish or grayish color, sometimes thin and translucent, sometimes thick and opaque, sometimes raised above and sometimes apparently on a level with the mucous surface, but lacking the size, the uniformity of surface, and the firmness of texture, necessary to mark them as unmistakable diphtheritic deposit. This transition or formative stage of the disease is catarrhal diphtheria, if the term can properly be used at all. When the membrane is organized and unmistakable, it is then "croupous diphtheria" (to use another of Oertel's cacological terms), whether it be a millimetre or a quarter of an inch in thickness, and whether it be of the size of a three-cent piece or cover the fauces. No case should be enumerated as one of diphtheria in which the membranous exudation is not so distinctly marked that it *could not possibly be anything else.* Of such cases the proper classification is into mild, severe, and malignant. *Very* mild cases, if enumerated, should be specially described, so that the reader as well as the narrator may be a judge of their genuineness.

hundred and forty-two. In the same three years I attended in my private practice twenty cases, nine of which occurred in the last year, making altogether two hundred and sixty-two cases, besides a few seen in consultation. My treatment having been, as I believe, progressive, and certainly attended with improving results, it is only of the more recent cases that I shall give some statistics. Some of my memoranda of the dispensary cases having been incomplete or lost, I have made my knowledge of the history of each case complete by much toilsome and careful recent personal inquiry; and of what I am about to offer I assert the absolute, or very nearly absolute, accuracy.

I should not omit to mention that I am indebted for kind and valuable assistance in the care of a number of these cases, and for the entire care of a few of them, to my friend Dr. D. C. Comstock. With this acknowledgment I shall, to avoid unnecessary multiplication of words, continue the narration in the first person.

Of the one hundred and twenty-four dispensary cases in 1875, eleven occurred in January, nine in February, three in March, eight in April, seven in May, five in June, nine in July, twenty-two in August, twenty in September, eight in October, fourteen in November, eight in December—showing the greatest prevalence in August and September.

Of these one hundred and twenty-four patients ninety-four recovered, and thirty died—or $24\frac{2}{10}$ per cent.

That this rate of mortality is at least fifteen per cent. less than the average from genuine cases of diphtheria during that year in that district will, I think, be admitted by those best qualified to judge; though, in the absence of full and accurate returns of the number of cases, it would be impossible to prove it statistically. But it yet gives no idea of the actual results of my treatment, which I fortunately can show statistically and accurately.

Of these one hundred and twenty-four cases, twenty-two passed under the care of other physicians, in most instances after a single visit only, and, in some, without the medicine

I prescribed having been procured—leaving one hundred and two that continued under my treatment. Of the one hundred and two that continued under my treatment, eighty-eight recovered and fourteen died.

Of the twenty-two who passed under the treatment of others, six recovered and sixteen died. The extreme badness of these latter results is partly to be accounted for by the fact that some of them were hopeless, and others severe cases, for which, on account of my unfavorable prognosis, other medical aid was called in.

This is not, however, true of all, fully half of them having been by no means bad when I saw them. Some of these left my care through dissatisfaction at my not using topical applications. The results in such cases, as I have since learned them, were particularly bad.

Of the fourteen who died under my care, one was moribund when first seen, surviving only two hours; one was already a hopeless case of laryngeal croup; two others were hopeless cases from extensive membranous affection and marked indications of blood-poisoning Deducting these, leaves ten deaths out of ninety-eight cases in which the treatment was tested with some degree of fairness, or a little over ten per cent.[1]

In judging of these results the very unfavorable conditions of much dispensary practice must be taken into consideration, in the lack of intelligent and efficient nursing, and the circumstance that in many of this class of cases the physician is only called in after the disease has already made great progress. Again, from endemic causes the general mortality of the disease in some portions of my district was simply frightful, the deaths of several children in one family, or of a considerable number in a single tenement-house, being an occurrence of which I often heard. It was within the limits of this district that that dreadful instance of the death of six children out of eight in one family, which attracted such gen-

[1] Two other bad cases might properly have been deducted, leaving only a little over eight per cent.

eral attention about a year ago, occurred. The records of the Board of Health show that this region contributed more than its full quota to the death-roll of 2,329 in this city in 1875.

While the population of the Twenty-first Ward is less than $\frac{1}{17}$ of that of the city by the last census, the deaths from diphtheria in it in 1875 were $\frac{181}{2329}$, or more than $\frac{1}{13}$ of all. Making proper allowance for the fact that nearly all of the diphtheria cases occurred in that half of the ward which is bounded on the west by Third Avenue, and which constitutes practically my visiting district, the mortality from the disease was decidedly greater in proportion to population here *than in any ward in the city.*

Again, these one hundred and twenty-four cases of diphtheria occurred among a total of 2,056 *different* patients visited by me in that year, as the books of the dispensary show. Considering the great prevalence of the disease in my district during that period, is this proportion a large one? And, considering its great fatality there, is it probable that my cases were "exceptionally mild ones?" Who would be likely to see a full proportion of the bad cases, if not the visiting physician of the dispensary? I have notes of the full name, residence, and date, of each of these cases, with the result in each, and shall be glad to submit them to the inspection of any one; and I challenge investigation of any or all of them.

Considering all drawbacks, the results in these cases cannot fail to be regarded as remarkably good, and decidedly favorable to the utility of the treatment employed.

That this system of treatment is capable of yielding still more brilliant results, when tested under the more favorable conditions that usually exist in private practice, I am now prepared to show.

From May, 1874, to the present time, I have treated in my private practice seventeen cases of diphtheria, with only one fatal result, and this one in consequence of a peculiar complication which I will presently describe. These cases were nearly all seen early. Eight of them were of decidedly severe tendency, two being only narrowly saved from a fatal termi-

nation; the others were mild. I give them in the order of their occurrence, as follows:

Case I.—Lottie B., aged four; Windsor Hotel, May 9, 1874. Duration of the disease (that is, from the first visit to the final disappearance of the membrane), four days.

Case II.—Will B., aged two and a half years; 646 Third Avenue, September 14, 1874; duration, four days.

Case III.—Edward D.,[1] aged eleven years; 245 East Twentieth Street, November 15, 1874; a case of severe tendency; duration, five days.

Case IV.—Michael D., aged thirty years; 124 East Twenty-seventh Street, February 25, 1875. This was one of the class of cases which some writers denominate croupous or membranous pharyngitis. Believing that all true membranous exudation on mucous surfaces is essentially diphtheritic,[2] I have enumerated it here. In this case, firmly-organized membrane on the tonsils and the posterior wall of the pharynx supervened in the course of a previously-existing subacute pharyngitis, and persisted, by partial renewals, for nine or ten days, the pharyngitis still continuing for some time afterward, with much constitutional prostration. Similar to this has been the character of the few other adult cases which I have seen. The adult mucous membrane, in health or in acute inflammation, is inhospitable to diphtheritic contagion-germs, but in these instances seemed to be prepared for their reception by its previously enfeebled and relaxed condition. Similar supervention often occurs upon the enlarged tonsils of children, and the angina of scarlatina, and, as I have seen in

[1] There had just been two fatal cases in this family.

[2] Many writers, following Bretonneau, describe a croupous or membranous sore-throat which they assume to be something distinct from diphtheria. This is, probably, a "distinction without a difference." These writers simply describe diphtheria supervening upon a sort of herpetic tonsillitis, as I have previously pointed out. I have seen some most malignant cases of diphtheria begin in just this way, and many mild ones that did not so begin. Dr. A. H. Smith reported a few months since to the Pathological Society an interesting adult case, which began as "membranous sore-throat," and terminated fatally as diphtheria.

some recent instances, upon that of the influenza now so prevalent. Some of these cases of compound disease have presented interesting peculiarities. While the previously-existing disease has evidently invited the diphtheritic contagion, and has occasioned the unusually long persistence of membrane, it has seemed to ward off the absorption of poison, and consequent constitutional infection.

Cases V., VI., VII.—Minnie, George, and Carrie S., 195 Third Avenue, ages seven, eleven, and nine years; March 30, April 12, and April 15, 1875; three cases of severe tendency, but kept in control by the very efficient application of the treatment; duration, four, three, and five days.

Case VIII.—Lizzie H., aged five years; 322 East Twenty-seventh Street, October 29, 1875; a case of severe tendency; duration, six days.

Case IX.—John B., aged five years; 236 East Twenty-seventh Street,[1] November 27, 1875; duration, four days.

Case X.—Mary B., aged two years, sister of the preceding; November 29. In this case the tonsils and soft palate rapidly became intensely inflamed, the uvula being much swollen. Vomiting, evidently of a reflex character, followed every act of swallowing. Various mild and unirritating remedies were administered internally and by local application, the iron-mixture having been found inadmissible. The use of the spray was attempted, but the resistance of the child made its efficient application impossible. For forty-eight hours all medicine and nourishment were at once rejected. Then the vomiting gradually ceased. Eggnog was taken and retained pretty freely. But the mischief done was irreparable. The membrane had spread very extensively. In the absence of nourishment, septic poison had been absorbed. Death ensued on the fourth day of the illness. This is the second instance, within my experience, of the same complication in very young children, with fatal result. In a few instances, in which it has occurred in older children, it has yielded to the use of the spray within a few hours.

[1] There had been a fatal case in this house a week or two before.

Case XI.—Walter A., aged five years; 212 East Twenty-fifth Street, December 9, 1875; a very severe and critical case, with vomiting, intense throat-inflammation, membrane on tonsils, faucial arch, and uvula, and nasal implication; duration, six days.

Case XII.—Mary L., aged two years; 309 East Twenty-fifth Street, December 31, 1875; a mild but distinctly membranous case; duration, three days.

Case XIII.—Mary C., aged seven years; 158 East Thirtieth Street,[1] February 9, 1876; a severe case, with membrane covering tonsils and uvula, nasal implication, croupy cough, and aphonia; duration, five days.

Case XIV.—John D., aged fourteen years; 220 East Twenty-ninth Street, February 20, 1876; duration three days, the tonsillar inflammation and enlargement continuing for some days afterward.

Case XV.—Will W., aged three years; 285 Third Avenue, February 25, 1876. This patient was attacked suddenly at evening with high fever and convulsions. The throat was not examined, but the tonsils showed externally slight enlargement. Three grains of calomel were prescribed, and my usual remedies. On the following morning the fever had almost entirely abated. The tonsils were enlarged and inflamed, with a small patch of membrane on each. Twenty-four hours later these had disappeared. The surface they had occupied was plainly denuded of its epithelium, though there was none of that loss of deeper substance characteristic of ulceration. This "exceptionally mild case," but for the treatment so promptly employed, would probably have been severe.

Case XVI.—Mary B., aged six years; 692 Third Avenue, March 13, 1876; duration, four days.

Case XVII.—Willie, brother of the preceding, aged four years, March 16th; duration, five days.[2]

My friend Dr. E. J. Darken, house-physician to Demilt Dispensary, furnishes me with a still better record of results.

[1] There had been a fatal case in this house a short time before.

[2] There had been a fatal case in this house a week or two before.

Since November, 1874, he has employed in his private practice the treatment which I have described. From that time to the present, he has treated twenty-four cases, without a single directly fatal result. Four were severe cases, with much nasal trouble and glandular swelling. Five others were severe cases, with membrane over the soft palate and uvula. The ages were as follows: two of one year; three of two years; two of three; three of four; one of five; four of six; one of seven; three of eight; one of nine; two of ten; two of twelve.

The duration of membranous affection was as follows: in one case, two days; in two, three days; in six, four days; in five, five days; in ten, six days.

One was followed by convergent strabismus of both eyes, another by amaurosis, and two by faucial paralysis.

One, a child, one year and a half old, died more than four weeks after the final disappearance of membrane, and more than three weeks after Dr. Darken's last visit, under the following circumstances: It exhibited no particular indication of illness, but continued weak; not so much so, however, that its parents thought it requisite to consult a physician. At length it was taken suddenly ill, with symptoms of extreme prostration, and Dr. Darken, on arrival, found it dying. No *post mortem* was made. This death was evidently due, not directly to the primary nor the constitutional disease, but to some resulting lesion. It happened to Dr. Darken, during this period, to be called to two moribund cases. One was not prescribed for; the other was prescribed for, but was dead before the medicine was procured.

About the middle of February, the time at which I formed the design of preparing this paper, my friend and neighbor, Dr. William E. Bullard, kindly consented to give me such assistance, in the care of all dispensary cases of diphtheria that should subsequently occur, as should enable us, by very frequent visits, to insure thoroughness in the application of the treatment. From that time to the present, we have had fourteen cases, one other, a mild case, having, after a single visit,

passed under the care of another physician. Every one of these has recovered. The majority have been, or at least have been kept by the treatment, rather mild. But among them have been three of the very worst cases that were ever known to recover. In two of these, Sarah and George Davis, aged nine and six years, residing at 324 East Twenty-sixth Street, the tonsils were greatly enlarged, and tonsils, pharynx, uvula, and soft palate, were covered with firm, thick, and very persistent membrane. The nasal implication was also extreme. Besides much offensive sanious, muco-purulent material, a membranous cast of one of the nasal passages, an inch long, was brought away by nasal syringing. Of another, a case of croup, I will give the particulars presently.

The ages were as follows: one of four years; one of three; four of four; one of five; two of six; one of seven; two of nine; one of ten; one of twenty-seven.

The duration of membrane was as follows: Two of two days; four of three days; two of four days; one of five days; one of six days; one of eight days; one of ten days; one of twelve days; one of twenty days.

I have thus presented three series of consecutive cases, my seventeen, Dr. Darken's twenty-four, and Dr. Bullard's and my fourteen, commencing in each series at the time when this method of treatment was effectively commenced, and extending, with no omissions of any case treated, to the present time, making fifty-five in all, with only one death directly resulting from the disease, and one other remotely.

I believe that in genuine membranous diphtheria very few, if any, such favorable results, from so large a number of unselected cases, have ever been reliably reported. They will naturally excite incredulity.[1] I can only say of these cases, as

[1] The opposition of opinions as to the necessary fatality of diphtheria is astonishing. I am informed that, of two eminent professors in one of our medical colleges, one teaches that a true case of diphtheria never recovers; the other states that, out of a considerable number which he has treated, he has rarely, if ever, lost one which he saw early. In a recent discussion in the Section on Obstetrics of the Academy of Medicine, as re-

of the others, that the name and address of each are at the service of any one, and that thorough investigation is invited. In my own cases, at least, even the correctness of the diagnosis can be substantially verified, for it has been my custom in private practice to show the throat to parents, and to point out the distinguishing features of the genuine disease.

Nor will it do to object that these have been "exceptionally mild cases." The number is too large, and they have occurred at a time and in localities in which the general mortality of the disease is well known to have been very great. Unmodified by treatment, their severity would doubtless have been fully equal to the average.

But while I thus defend my statistics against possible unjust criticism, I freely admit that it is only by the concurrence of unusual good fortune that this last group are so extremely favorable. I wish to state explicitly that I do not claim that this treatment will as a rule cure such cases as the two which I have just described. I do claim that it will, in the great majority of cases, when early applied, prevent the disease from becoming very severe, greatly shorten its duration, and avert serious systemic infection.

I believe I am warranted in claiming for it something more. Out of fully one hundred cases, including Dr. Darken's, in which the spray of lime-water and carbolic acid has been employed, there has been no instance of the subsequent occurrence of serious laryngeal complication, although in several of them it has been threatened by croupy cough, hoarseness, and aphonia. That the inhalation of the spray may have acted as a preventive in some of these is, I think, not improbable.

In a recent case of Dr. Darken's, this complication was

ported in the *Medical Record* of April 8th, Dr. F—— stated that "he had not saved a single case in which the membrane had become formed" (it would be interesting to know what treatment he has employed), "and inquired of Dr. C—— if he could point to any cases of diphtheria which had got well under his plan of treatment. Dr. C——, in reply, cited six cases in one family, with recovery in every case."

more than threatened, all the symptoms having been well marked, but having disappeared in about two days.

On Friday, February 19, 1876, I visited from the dispensary Rebecca Hewitt, aged four years, a robust child, at 338 East Thirty-third Street. The day before, as her mother said, she had begun to have a croupy cough, to which in the night had been added difficulty in breathing. I found her suffering from high fever, brassy cough which gave her great pain in the larynx, and stridulous, labored breathing, with marked depression over the clavicles at each inspiration. The throat was congested, and the tonsils much swollen, and on each of the latter was a patch of firmly-organized membrane. Believing that the case must end fatally in from twenty-four to forty-eight hours, I yet prescribed the iron and the salicylic-acid mixtures at half-hour intervals; had an atomizer procured, and directed the spray to be applied almost constantly. Fortunately, the child favored the operation by opening her mouth widely and inhaling the spray.

The next day I found the symptoms aggravated, both tonsils now being nearly covered with membrane, the aphonia complete, and the cough extremely harsh and painful, sometimes bringing up muco-purulent material in considerable quantities. There were high fever and violent headache. I prescribed five grains of calomel, the other remedies to be continued.

On the third day I called, expecting to find my patient dead, but on the contrary found her a little better. The dyspnœa persisted, but the sound of the cough was softer. The membrane upon the tonsils was loosening.

On the fourth day the case was seen by Dr. Bullard. The croupy symptoms were somewhat mitigated, but still grave. The membrane had disappeared from one tonsil and partially from the other.

On the fifth day, further improvement; membrane entirely gone from the throat.

On the sixth day all the symptoms were much worse, the patient having taken cold from a sudden change in the weather.

From this to the eleventh day gradual improvement oc-

curred. During all this period the labored, stridulous respiration, with depression over the clavicles and capillary congestion, the harsh, painful cough, and the aphonia, persisted without any intermission. After the eighth day bloody material was coughed up profusely. The spray was applied throughout at very frequent intervals. After the ninth day inhalations of the vapor of hot-water containing the compound tincture of benzoin were also used.

On the thirteenth day, all the bad symptoms had nearly disappeared; but bloody material was still coughed up for several days.

The early disappearance of the membrane from the throat, and the absence throughout of indications of constitutional infection, are circumstances to be noted.

In my previous cases of this kind I had employed the usual steam-treatment, with no successful result. They nearly all illustrated the truth of Prof. Jacobi's remark that "the fact that it" (moist heat) "softens healthy tissue as well as morbid exudations, appears to favor the penetration of the poison into deeper layers." It has also seemed to favor the rapid superficial extension of the diphtheritic disease in the fauces, this having ensued in several of my cases which previously manifested no such tendency. I hope that, in the discussion which is to follow, some facts may be elicited as to the actual results of a mode of treatment in diphtheria which has been so extensively introduced among us by Oertel's article in Ziemssen's "Cyclopædia."

It is a truism that, however plausible the theory that suggests a method of treatment, the only touchstone of its merits is its actual results. So many and so well known are the elements of uncertainty attending even these, that they can become a reliable basis of comparison only when multiplied by a large number. The materials for such statistics exist among us in ample abundance. Had they been utilized, the young or inexperienced practitioner would hardly be so sorely perplexed as he must be to-day among a multitude of diverse teachings. Let us for a moment imagine that, during these

years of the fatal prevalence of diphtheria, there had existed among us a properly-accredited society or bureau for the collection of such statistics. Let us imagine that to this bureau all physicians had been requested to contribute particulars of the disease and of the treatment employed, in every case of recovery and of death. With such a request many would doubtless have complied. Had this been done we should now be in possession, not of a few detached and fragmentary data, but of masses of authoritative facts. Some of these would present unmistakably the results of modes of treatment that are simply harmless; others, of those that are beneficial; and still others might, like beacon-fires on a dangerous coast, warn us from the employment of methods that are positively injurious.

Shall the golden harvest of vital facts that is perpetually maturing in our midst remain longer ungarnered?

Postscript.

In illustration of some of my *negative* recommendations, which are scarcely less important than the positive ones, I append two cases out of many that might be cited:

Case I.—On a Tuesday, in August, 1875, I visited from the dispensary Mary Lyons, aged seven years, at No. 230 East Twenty-ninth Street, which block was infested with malignant diphtheria. The local disease was already extensive when I first saw her, and rapidly spread so that the pharynx, fauces, and nearly the whole roof of the mouth, were covered with thick membrane. I employed my usual treatment, with no quinine or stimulants, and milk as a diet, which she took freely. She continued to sit up much of the time; her pulse was good, and she exhibited very little appearance of blood-poisoning.

She was in this condition on the following Sunday, when, going in at an unexpected time, I found another physician visiting my patient. I at once resigned the case to him. This doctor, who was a very intelligent and courteous gentleman, expressed astonishment (as well he might) at seeing a case

with so extensive exudation exhibit so little constitutional disease. But did he let this remarkable phenomenon teach him its proper lesson? Instead of "letting well enough alone," he proceeded, in spite of my mild remonstrance, to "sustain" the patient by ordering alcoholic stimulants.

I recently learned the further history of the case from the mother. On the next day the doctor made some kind of topical application. The child "went right down." Five other doctors were called in by the distracted father. The child probably got stimulants and quinine enough, but died about a week later, covered with purpuric spots. The sudden change in the phenomena of this case needs no comment.

Case II.—In November, 1875, I visited from the dispensary Kate and John Mulhern, aged six and eight years, at No. 696 Second Avenue. They were cases of about average severity, both tonsils in each being enlarged and covered with membrane. I prescribed the tincture-of-iron mixture, to be taken every half-hour. The mother asked me if that was all I was going to do for her children. She said the doctor who was attending some cases in the same house came several times a day and made some application with a brush. My further services were not desired.

Calling a few weeks ago, in pursuit of statistics for this paper, I found that the family had moved away. The neighbors told me that these two children and a third, subsequently taken ill, had died. It may be said that they might have died under my treatment. This is, of course, possible, but is extremely improbable, so widely would it have been at variance with my usual results in such cases.

If any shall adopt some of my methods only to make them *part* of a treatment in which quinine, alcohol, etc., or topical brushing, play an important part, I predict that they will be the very ones who will report that my system is a failure!

In the discussion which followed the reading of this paper, Dr. J. C. Peters made a most suggestive statement. Having mentioned his concurrence with the views I had expressed, and the fact that he had for years employed a treatment similar in essentials to my own, he added:

"During my illness, my practice passed into the hands of other physicians, and I was surprised to hear of the number of malignant cases of diphtheria that occurred in it. After I recovered and resumed my practice, they *ceased to occur.*"

In conclusion, I wish to place in this connection a brief extract from the remarks of my friend Dr. H. T. Hanks, in the course of the same discussion, as reported in the *Medical Record* for March 25th, for the sake of the corroborative testimony and a most important practical suggestion which it contains. Dr. Hanks said:

"Dr. Billington's success was truly remarkable, for he well knew the type of the disease as it had appeared in the Twenty-first Ward, having had, in his private practice during the last five years in that district, from twenty to thirty cases every year. He knew that many of the cases attended by Dr. Billington had been severe, and not a few malignant. Therefore, when the large per cent. of recoveries was considered, a cause must be looked for; and he believed two excellent reasons could be found for this satisfactory result. One was the kind of medicaments used locally and internally, and the other was the great care he bestowed in teaching the parents or nurses the *proper manner* of administering the remedies presented. This carrying out to the letter every little detail has had much to do, more than many have been led to suppose, in the cure of diphtheria.

"He wished he could sufficiently emphasize the vast importance, in treating diphtheria, of careful attention to the minutiæ. Many had been, and still were, in the habit of looking at the patient's throat, writing a prescription, ordering a gargle every few hours, and the nose to be syringed twice a day, believing that their directions would be followed. He knew, however, that one-half of the best class of patients even did not receive the full benefit of the medicaments, through lack of proper, intelligent nursing."

Dr. Hanks also mentioned that experience had taught him to employ a treatment similar in principle to mine, and with very good results.

THE CAUSE OF ROTATION IN LATERAL CURVATURE OF THE SPINE.

By A. B. JUDSON, A. M., M. D.,

Read April 6, 1876.

The earliest description[1] of the rotation which takes place in lateral curvature of the spine was written by Andrew Dods, M. D., London, who published his "Observations on the Rotated or Contorted Spine, commonly called Lateral Curvature," in January, 1824. Mr. Bampfield[2] described it later in the same year. Dr. Dods's observations, and the theory which he constructed, are described by Mr. Adams,[3] who further states that Mr. John Shaw, in the latter part of 1824, mentioned rotation; that Bouvier, of Paris, describes it in 1836; and that Sir Charles Bell in 1842, and M. Jules Guérin in 1843, refer to it as one of the incidents of lateral curvature.

Rotation is a constant feature in the ordinary form of lateral curvature, whether the disease be incipient or confirmed.

[1] I am indebted to a Fellow for a reference to the following: "There are others also, wherein the vertebræ not only grow crooked on the Ribs, but turn in a manner a little round on themselves; and there is something in this Sort of Deformity that requires particularly to be observed, and that is, that the oblique Apophyses (articular processes) of the convex side draw close to the concave side."—("The Compleat Surgeon. Second Part. M. Le Clerk. Englished from the French." London, 1710, pp. 286.)

[2] "Essay on Curvature and Diseases of the Spine." By R. W. Bampfield. London, 1824, pp. 170.

[3] "Lectures on Lateral and other Forms of Curvature of the Spine." By William Adams. London, 1865, pp. 116, 117, 175–177.

Dr. Dods[1] believed that it was the primary movement of lateral curvature in every instance. Mr. John Shaw says,[2] "It always accompanies even the slightest degree of serpentine or lateral curvature." Sir Benjamin Brodie says,[3] "At a very early period, and even before the lateral curvature is very distinct posteriorly, the bodies of the vertebræ are actually twisted on one side."

Mr. Barwell says,[4] "With every lateral bend of the spine, whatever be its cause, a commensurate amount of rotation is combined."

Mr. Adams states[5] that "it occurs in all cases of lateral curvature as a constant and essential condition of this affection."

There is a variety of lateral curvature, however, in which rotation is absent. Dr. Henry Dick says:[6] "Real lateral curvature of the spine is only found directly after pleurisy, with exudation, or after abscesses in the costal region. It is generally produced by post-inflammatory contraction and position during the pleurisy. All other cases of lateral curvature of the spine are more or less combined with rotation or twisting of the spinal column itself."

Dr. Little says,[7] "The absence in these pleuritic contractions, or true lateral curvature of the spine, of any rotation, or twist, . . . is very striking."

Rotation is always toward the convexity of the lateral curve as to the bodies of the vertebræ, and toward the concavity as to the spinous processes. In other words, if the vertebral column curves to the right, the spinous processes incline toward the left; if the column curves to the left, the processes

[1] Bampfield, *op. cit.*, p. 204.

[2] Adams, *op. cit.*, p. 119. [3] Ibid., p. 120.

[4] "Lateral Curvature of the Spine." By Richard Barwell. London, 1870, p. 62.

[5] "Spine, Curvatures of." Cooper's "Surgical Encyclopædia." Article by William Adams. London, 1872, p. 619.

[6] *Medical Times and Gazette*, London, August 20, 1864, p. 203.

[7] "Spinal Weakness and Spinal Curvatures. W. J. Little, M.D.' London, 1868, p. 120.

incline toward the right. In the compensating curves the same rule holds good.

The degree of rotation is in proportion to the degree of curvature. In that vertebra which has departed farthest from the normal line the rotation is the greatest, and it diminishes above and below as the vertebræ approach the normal line.

In confirmed lateral curvature, the hard and soft parts undergo important structural changes. The ligaments become lengthened or shortened, the muscles are hypertrophied or atrophied, and the bones are modified in shape, either by pressure upon each other, or by the contraction of muscles whose points of origin and insertion have lost their normal relation to each other. The changes in the bones are most marked in the articular processes and ribs, the alterations in the former being produced by pressure upon each other, and in the latter by muscular action in abnormal directions.

The cause of the rotation of the vertebræ in lateral curvature has been the subject of much study and speculation. Dr. Bauer calls it "one of the most vexatious questions." He has adopted the explanation proposed by Prof. Herman Meyer after an elaborate series of experiments.[1] Prof. Meyer discovered that the anterior portion of the spinal column possessed great expansibility, and the posterior portion great compressibility, and inferred that, when the lateral curvature took place, the posterior portion of the column would seek the concavity, and the anterior portion the convexity of the curve.

Mr. Alexander Shaw, in 1832,[2] described certain structural changes which appeared in the articulating processes in lateral curvature of the spine, and found in these changes an explanation of the occurrence of rotation. Mr. William Adams[3] adopts the views of Mr. Shaw.

Mr. Bampfield says,[4] "The contortion or serpentine turn

[1] "Lectures on Orthopedic Surgery." By Louis Bauer. New York, 1868, pp. 158–160.

[2] "Medico-Chirurgical Transactions," vol. xvii., p. 467.

[3] Cooper's "Surgical Encyclopædia," p. 620.

[4] Bampfield, *op. cit.*, p. 204.

of the spine seems referable to malformation or irregular growth of the vertebræ, or of the ribs inserted into them."

Dr. Henry Dick proposed an explanation of rotation to the British Medical Association which is given in the following words from an abstract of his paper:[1] "After much observation and numerous experiments, Dr. Dick has arrived at the conclusion that the twisting is purely the mechanical consequence occasioned in the deviated or curved spine by bending it forward. The spine in some respects resembles a strong, elastic stick, or column, which, if pressed at both extremities more strongly than it can bear, forms a bow, and in that condition will not bend backward but will rotate at the central points of the bow. If a strict examination be made into the pathological anatomy of spinal curvature, the same rule above mentioned holds good."

Dr. Bigelow, following, in the main, the doctrines of M. Guérin, says:[2] "The principle of torsion is illustrated by an attempt to bend a blade of grass, or a flat, flexible stick, in the direction of its width. The centre immediately rotates upon its longitudinal axis to bend flatwise in the direction of its thickness. In the same way the spine laterally flexed turns upon its vertical axis to yield in its shortest or antero-posterior diameter."

The views of Mr. Barwell are based[3] on a disparity in the action of certain pairs of muscles attached to the vertebræ. The disparity of action is caused by a difference in the weight of the arms and in the capacity of the lungs.

Dr. Dod's theory[4] of the cause of rotation in the lumbar region is very similar to that of Mr. Barwell, both of these writers supposing that it is caused by the action of the sacro-lumbalis, longissimus dorsi and quadratus lumborum, the former believing that rotation of the lumbar vertebræ, and

[1] *Medical Times and Gazette*, August 20, 1864, p. 203.

[2] "Orthopedic Surgery." Boylston Prize Dissertation, 1844. By Henry J. Bigelow. Boston, 1845, p. 168.

[3] Barwell, *op. cit.*, pp. 35 and 64.

[4] See Bampfield, *op. cit.*, p. 204.

the latter that rotation of the dorsal vertebræ[1] (by the serratus magnus), is the primary movement of lateral curvature.

Contrasting Mr. Barwell's view, that rotation is the primary movement of lateral curvature, and the generally accepted view, that it is a sequel or incident, Mr. Gant says,[2] "Both these views cannot be true, but both are entitled to consideration."

In the present unsettled state of the question, I propose an explanation as follows:

The rotation of the vertebra in lateral curvature of the spine consists in an unequal lateral displacement of the body and the spinous process, the former being free from lateral attachments and therefore departing from the median plane, while the latter is held in the median plane by its muscular and fibrous attachments.

The muscles attached to the vertebral column may be divided into two groups.

The first group includes all those muscles which do not serve to connect the vertebral column directly with the shoulders, the thoracic and abdominal parietes and the hips. They are so situated as to their direction and attachments that they lie—1, between contiguous or adjacent vertebræ, as the longus colli or the rotatores spinæ; 2, between the vertebral column and the head, as the recti capitis, or 3, between the vertebral column and the lower extremities, as the psoas magnus. Their action is chiefly parallel to the axis of the spinal column. Some of them rotate the spine, but they do so because they form oblique lateral muscular connections between different parts of the spine, and may be called intrinsic rotators when compared with some of the muscles of the second group, which may be called extrinsic rotators.

The second group includes those muscles that are so situated, in respect to their direction and attachments, that they serve to connect the vertebral column with the shoulders, the thoracic and abdominal parietes and the hips.

[1] Barwell, *op. cit.*, p. 37.

[2] "The Science and Practice of Surgery." By F. J. Gant. London, 1871, p. 875.

They are the following: trapezius, latissimus dorsi, rhomboideus minor, rhomboideus major, serratus posticus superior, serratus posticus inferior, scalenus anticus, scalenus medius, scalenus posticus, levator anguli scapulæ, levatores costarum, crura of the diaphragm. Their action is in great measure at right angles to the axis of the spinal column.

The trapezius is attached, on the one hand, to the ligamentum nuchæ, and thus indirectly to the cervical spinous processes, and to the spinous processes and supra-spinous ligament from the seventh cervical to the twelfth dorsal vertebra, and, on the other hand, to the clavicle and spine of the scapula. Both the trapezii are in action for steadying the shoulder during every considerable movement of one or both of the upper extremities. If both arms are called into action, the trapezii antagonize each other and keep the spinous processes, to which they are attached, in the median plane of the trunk. If but one arm is in action the antagonism of the trapezii still keeps the spinous processes in the median plane of the trunk, as may be seen in the following sequence.

Any force exerted by the contraction of the deltoid is transmitted first to the scapula and clavicle, thence to the trapezius of that side, thence to the spinous processes, thence to the trapezius of the other side, thence to the scapula and clavicle of that side, and is expended on the vis inertiæ of the shoulder and arm, and, if necessary, on some firm support grasped by the upper extremity on that side.

This transmission of force from side to side takes place during every considerable action of one or both of the upper extremities, and must always have for its incidents an antagonism of the trapezii, and a retention of the spinous processes to which they are attached in the median plane.

The latissimus dorsi is attached, on the one hand, to the spinous processes of the lower six dorsal and the lumbar vertebræ, and, on the other hand, to the humerus. Any considerable movement of the arm which implies the action of either latissimus dorsi throws the other into action, with a resulting

antagonism which holds the spinous processes, to which they are attached, in the median plane.

The rhomboidei are attached, on one hand, to the spinous processes from the seventh cervical to the fourth dorsal vertebræ, and, on the other hand, to the scapulæ. They, like the two muscles above mentioned, are thrown into an antagonistic action with every considerable motion of one or both of the upper extremities, and their action also keeps the spinous processes, to which they are attached, in the median plane.

The serratus posticus superior is attached, on one hand, to the ligamentum nuchæ, and to the spinous processes of the last cervical and two or three upper dorsal vertebræ, and, on the other hand, to the ribs from the second to the fifth inclusive.

The serratus posticus inferior is attached, on one hand, to the spinous processes of the last two dorsal and the two or three upper lumbar vertebræ, and, on the other hand, to the four lower ribs.

The serrati are muscles of respiration and antagonize each other from side to side, assisting to keep the spinous processes, to which they are attached, in the median plane.

From the above review of the action of the trapezius, latissimus dorsi, rhomboidei and serrati postici, it is evident that one of their functions is to hold the posterior portion of the vertebral column in the median plane. In this function they act to great advantage from being attached to the extremities of the spinous processes. The other muscles of the second group, the scaleni, the levator anguli scapulæ, the levatores costarum, and the crura of the diaphragm, do not act with so much advantage from their being attached, with the exception of the crura of the diaphragm, to the transverse processes, but they all assist, to a certain degree, in holding the posterior portion of the vertebral column in the median plane. The crura of the diaphragm, attached to the bodies of the vertebræ, do not oppose the action under consideration.

In the above brief description of the action of the muscles which antagonize their opposites and hold the spinous proc-

esses in the median plane, I have fallen very far short of mentioning all the actions of these muscles which have this effect. The muscles enumerated are called into action in nearly all the movements of the body. They are thrown into action in standing, in locomotion, and in the performance of all the considerable movements of the upper extremities and of the head. They are active in respiration, in the expulsive movements of the thorax and abdomen, and in the flexion and extension and lateral curving of the trunk, and in all these actions they antagonize each other in pairs, and keep the spinous processes of the vertebral column in the median plane.

Moreover, they have for their auxiliaries many powerful muscles which are not included in the list of those which directly connect the spinous processes with the shoulders, the thoracic and abdominal parietes, and the hips.

For example, the rhomboidei are assisted by the serratus magnus. Mr. Barwell says,[1] "These muscles may be regarded as one broad fleshy layer, which, arising from the upper part of the spine, sweeps round and embraces the back and sides of the chest; and in this view the intervening base of the scapula is to be considered merely as an intersection, like the semilunar lines in the abdominal rectus." The rhomboidei and the serratus magnus are in turn assisted by the obliquus externus, which may be considered the third section of one broad fleshy layer sweeping round from the spinous processes of the upper dorsal region to the hips, Poupart's ligament, and the linea alba.

For other examples, the latissimus dorsi is assisted, in its action on the spinous processes of the lumbar region, by the sacro-lumbalis, and the trapezius is assisted in its action on the spinous processes of the dorsal and cervical regions by the pectorales, through the intervention of the humerus and scapula.

The ligaments, aponeuroses, and fasciæ attached to the vertebral column may be divided into two groups.

The first group includes those which do not serve to con-

[1] Barwell, *op. cit.*, p. 30.

nect the vertebral column with the shoulders, thoracic and abdominal parietes, and hips.

They lie—1, between contiguous or adjacent vertebræ; 2, between the vertebral column and the occipital bone, or 3, between the vertebral column and the pelvis.

The second group includes those which connect the vertebral column with the thoracic and abdominal parietes and the hips.

They are the vertebral aponeurosis, lumbar aponeurosis, or fascia, ligaments connecting the bodies of the vertebræ and the ribs, ligaments connecting the transverse processes and the ribs, ligamentum arcuatum externum.

The lumbar aponeurosis, or fascia,[1] is attached, on the one hand, to the spinous processes of the lumbar vertebræ, and, on the other hand, to the crest of the ilium. With its opposite, it holds the spinous processes of the lumbar vertebræ in the median plane.

The vertebral aponeurosis[2] is attached, on one hand, to the spinous processes of the dorsal vertebræ, and, on the other hand, to the angles of the ribs. With its fellow, it serves, but less effectively than the lumbar aponeurosis, to keep the spinous processes of the dorsal region in the median plane.

The ligaments mentioned serve the same purpose to a certain degree. Being attached to the transverse processes, and, therefore, acting at a disadvantage, compared with the aponeurosis mentioned, which are attached to the spinous processes, they still assist in holding the posterior portion of the spinal column in the median plane. One group, the ligaments connecting the heads of the ribs with the vertebræ, are attached to the bodies of the vertebræ, but so far back that they do not oppose the action under consideration.

While the spinous processes of the vertebral column are held in the median plane of the trunk by the strong and active fibrous and muscular structures which are attached, on one

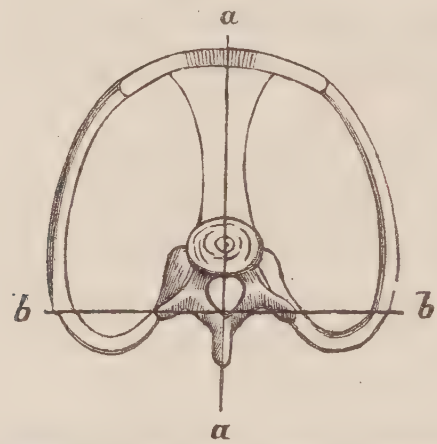
[1] "Anatomy, etc." By Henry Gray, Philadelphia, 1862, p. 270, fig. 160; p. 285, fig. 165.

[2] Gray, *op. cit.*, p. 274, fig. 160.

hand, to the posterior portion of the spinal column, and, on the other hand, to the shoulders, the thoracic and abdominal parietes, and the hips, the anterior portion of the vertebral column is comparatively free from fibrous and muscular attachments.

A glance at the subjects in the dissecting room, where bodies in a late stage of dissection are often divided near the dorso-lumbar articulation, or an inspection of the thoracic and abdominal cavities, after evisceration in an autopsy, will show the anterior portion of the vertebral column, composed of the bodies of the vertebræ, almost entirely free from attachment, and standing out prominently from the mass of the thoracic and abdominal parietes.

FIG. 1.



SECTION OF HEALTHY CHEST (ADAMS).
Showing the position of the body of a vertebra in the thoracic cavity.

The muscles attached to the bodies of the vertebræ are the following: longus colli, crura of the diaphragm, psoas magnus, psoas parvus.

The fibrous structures attached to the bodies of the vertebræ are the following:

Anterior common ligament, posterior common ligament, intervertebral substance, some of the ligaments connecting the axis, atlas, and occipital bone, ligaments connecting the heads of the ribs and the bodies of the vertebræ, ligamentum arcuatum internum, iliac fascia, root of the mesentery, ligamentum latum pulmonis.

The muscles and ligaments above enumerated are so situated, in regard to their direction and attachment, that they have no power directly to keep the anterior portion of the vertebral column in the median plane of the trunk.

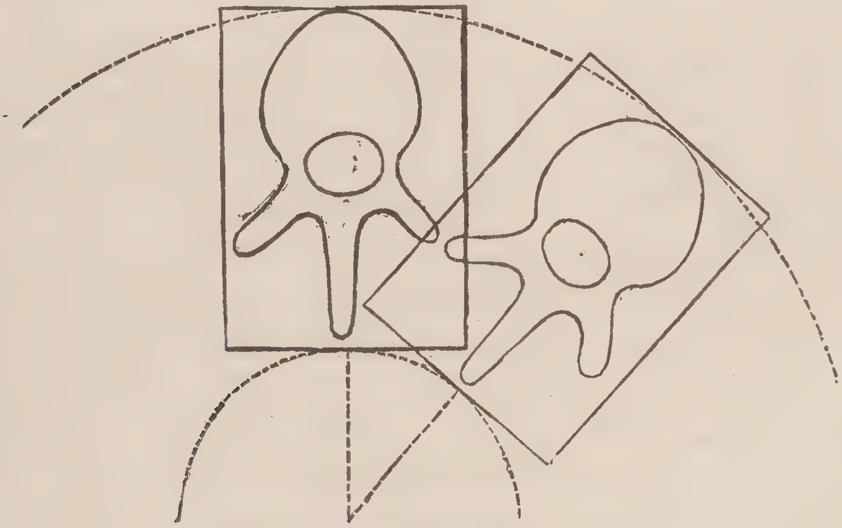
The bodies of the vertebræ have motion upon each other by the intervention of the intervertebral substance. They are unlimited in lateral motion, except by the intervertebral substance, the anterior and posterior common ligaments, their slight visceral and costal connections, and from being connected, by their pedicles, with the posterior portion of the vertebral column, the component parts of which are bound together by ligamentous and muscular structures.

When the median plane of the trunk curves to the right or the left, under the vertical pressure which is the direct cause of lateral curvature, their freedom from control allows the bodies of the vertebræ to fall away from the median plane, to the right or the left, while the posterior portions of the column are held in the median plane by their muscular and fibrous attachments. This deportment of the two components of a vertebra, its anterior and posterior portions, is rotation in the vertebra as a whole.

In mechanics, rotation in a given body may be on a central axis, a peripheral axis, or a remote axis.

The rotation of a vertebra in lateral curvature is rotation on a remote axis.

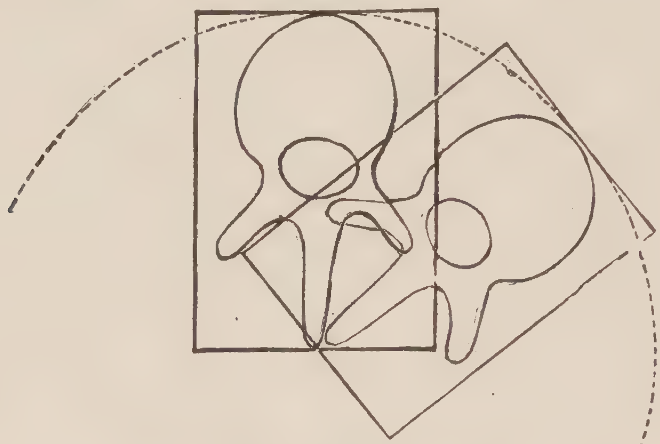
FIG. 2.



ROTATION ON A REMOTE AXIS.

Considering, first, a single vertebra, the spinous process describes the arc of a smaller circle than the body of the vertebra. Considering, secondly, the vertebral column, the spinous processes and the bodies of the vertebræ curve, together with the median plane, toward the same side, the right or the left, as the case may be, the spinous processes remaining in the median plane, and the bodies departing from it.

Fig. 3.



Rotation on a Peripheral Axis.

If the rotation were on a peripheral axis, considering a single vertebra, the spinous process would be the centre on which the body describes an arc. Considering the vertebral column, the median plane would be stationary, the bodies would curve to the right or the left of the median plane, and there would be no curvature in the line of the spinous processes.

If the rotation were on a central axis, considering a single vertebra, the spinous process and the body would describe opposite arcs of a circle. Considering the vertebral column, the bodies and the spinous processes would remain in the median plane, which would revolve on a vertical axis in such a manner that the spinous processes would curve to the right in that portion of the plane posterior to the axis of revolution, and the bodies would curve to the left in that portion of the plane anterior to the axis of revolution, or, vice versa, the processes would curve to the left and the bodies to the right, a condition of the parts different from

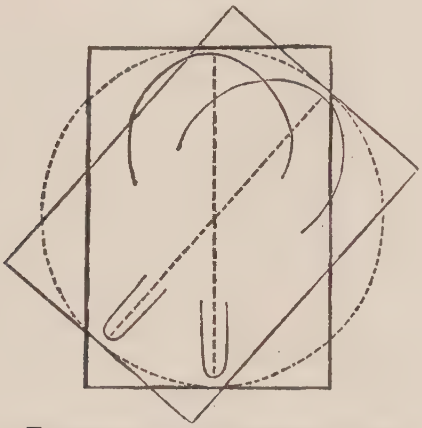
O. G. MASON, PHOTO.

E. BIERSTADT ALBERTYPE.

LATERAL CURVATURE OF THE SPINE

what is found in lateral curvature, where the spinous processes and the bodies curve together to the right or the left.

FIG. 4.



ROTATION ON A CENTRAL AXIS.

The production of rotation on the theory here presented is well illustrated by a preparation, the construction and action of which are shown in the Plate. The rod is made of brass tempered by the hammer. It has lateral but not antero-posterior flexibility. The perforated diaphragm is of pasteboard, and is glued to the pedicles and laminæ of the vertebra in such a way as not to interfere with the rotatory motion of the vertebræ upon each other. The elastic cords are stationer's elastics used in filing papers. To produce lateral curvature of the column, with rotation of the vertebræ, the knob at the summit of the apparatus is to be depressed. Double curvature, with rotation in each curve, may be produced by confining one of the dorsal vertebræ with the silk check loops, and depressing the knob as in the first instance.

The explanation here proposed answers all the difficulties that have arisen in the study of the phenomena of rotation in lateral curvature.

Rotation occurs in all forms of lateral curvature except that caused by collapse of the ribs from pulmonary or pleural disease. In these cases, rotation is absent, because the muscles attached to the spinous processes, on the side of the concavity, the trapezius, latissimus dorsi, rhomboidei, and serrati postici, together with the vertebral aponeurosis, all have their points of origin and insertion respectively approximated by a col

lapse of the chest on that side, and the spinous processes move toward the opposite side, together with the bodies of the vertebræ, and rotation is annulled.

The turning of the spinous processes toward the concavity in the primary and compensating curves, the increase and diminution in the degree of rotation in proportion to the increase and diminution in the lateral curvature, are phenomena which readily accord with the explanation proposed.

The structural changes which take place in lateral curvature are the changes which might be expected from curvature accompanied by rotation produced in the manner above described.

The distinguishing feature of the explanation of rotation here proposed is the recognition of the fact, heretofore overlooked, so far as I am aware, that the posterior portion of the vertebral column, being a part of the dorsal parietes of the chest and abdomen, is confined in the median plane of the trunk, while the anterior portion of the column, projecting into the thoracic and abdominal cavities, and devoid of lateral attachments, is at liberty to, and physiologically does, move to the right and left of the median plane.

The explanation of rotation on the ground that the anterior portion of the column has expansibility, and the posterior portion compressibility, depends on the fact that, if a flexible column is bent, its component parts, if movable upon each other, will rotate, until they bring into the concavity that portion of the column which offers the least resistance to the bending, a fact which is also the basis of that explanation which compares the spine affected with lateral curvature, to a blade of grass, or a flat, flexible stick, bent in the direction of its width, or that which compares it to a strong, elastic stick, or column, pressed at both extremities more strongly than it can bear.

Considering the proportionate length and thickness of the spinal column, the rotation which its bodies perform is too short, and sharp, and decided to be produced, either by the different endowments of its anterior and posterior portions as

to the physical qualities of compressibility and expansibility, or by the inclination of the column to rotate until the side which opposes least resistance to the bending is in the concavity of the curve.

A simple test will throw out of the question the suggestion which compares the spine to a blade of grass, or a flat, flexible stick, bent in the direction of its width. Take a slip of cardboard, one half inch in width, cut from the side of an ordinary visiting card. If an attempt be made to bend it in the direction of its width, it will rotate either to the right or the left. Now, bend the slip of card in the direction of its thickness, giving it as much curvature as the normal spinal column has in the dorsal region in the antero-posterior direction, and, retaining this curve, again attempt to bend the slip of card in the direction of its width. It will be seen that the side of the card which corresponds with the posterior portion of the spinal column, or with the spinous processes, rotates toward the convexity, instead of toward the concavity.

The theory, which is based on the changes which take place in the articulating processes, is open to the grave objection that the changes in question are found only in confirmed cases, while rotation is incident to all stages of lateral curvature.

The theory that rotation depends on abnormal muscular contraction seems to lead inevitably to the conclusion that the same abnormal muscular contraction is the cause of the lateral curvature. This theory differs essentially from the explanation presented in the preceding pages. If the latter is generally accepted it will simplify very materially the question of the cause of lateral curvature, in its ordinary form, by rendering it more than probable that the immediate cause of the curvature is an inability to support the superincumbent weight. What tissues are at fault, and why they fail at a certain point of development, and in females more often than in males, are questions which are not included in the subject of this paper.

Rotation has been thought to occasion great difficulty in the mechanical treatment of lateral curvature.

Dr. Prince says, after mentioning the effect of rotation on the ribs,[1] "It is this change in the curvature of the ribs that constitutes one of the greatest obstacles to the restoration of the proper form in confirmed cases."

Mr. Little says,[2] "This abnormal rotation constitutes the principal obstacle to the cure of severe lateral curvature."

Bouvier[3] describes rotation as existing only in the severe forms of curvature. Mr. Tamplin[4] supposed that it occurred only in the more severe cases, and Mr. Brodhurst[5] states that it commences as soon as curvature becomes confirmed.

The treatment of lateral curvature is indeed beset with difficulty, not so much from the presence of rotation, however, as from the fact that the portion of the column which departs furthest from the normal position cannot, from its situation within the cavity of the trunk, receive direct mechanical support. Lateral curvature has a resemblance to Pott's disease of the spine in that, in both of these affections, the anterior portion of the column is subject to a departure from its normal conditions. In one case the vertebral bodies are carious; in the other they are thrown out of their normal position: in the one case the caries is prolonged and extended by the weight of the parts above; in the other case the deviation is directly produced, and aggravated by the same superincumbent weight. In Pott's disease the morbid process is arrested by transferring the injurious weight from the anterior and diseased portion to the posterior and sound portion of the spinal column. In lateral curvature would not a degree of relief be afforded by a similar application of antero-posterior force, by which a part of the weight would be transferred to the posterior portion of the spinal column, which is prevented, by its muscular and fibrous attachments, from deviating far from its normal position?

[1] "Orthopædics." By David Prince, M. D. Philadelphia, 1866, p. 96.

[2] *Op. cit.*, p. 366.

[3] See Adams, "Lectures," *op. cit.*, p. 119.

[4] "Lectures on Deformities." By R. W. Tamplin. Philadelphia, 1846, p. 262.

[5] "Curvatures of the Spine." By Bernard E. Brodhurst. London, 1864, p. 59.

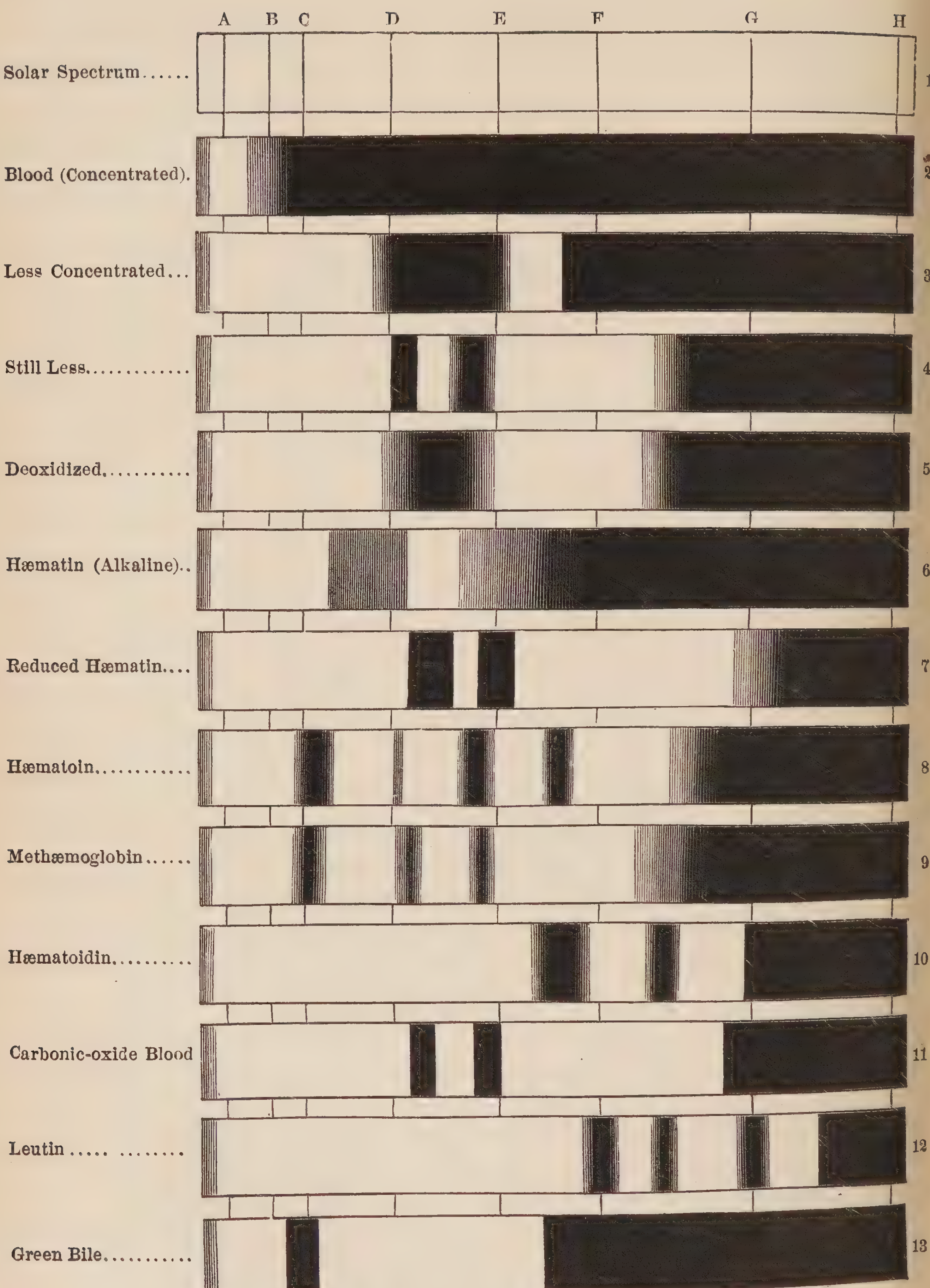


CHART OF SPECTRA.

THE SPECTROSCOPE, ITS VALUE IN MEDICAL SCIENCE.

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Read April 20, 1876.

Mr. President and Fellows of the Academy:

In 1670, or thereabout, Newton discovered the compound nature of white light. That is, he found that when a beam of pure sunlight is allowed to pass through a prism it undergoes two changes. First, the direction is altered, or the rays are refracted; and, secondly, it is no longer white light but broken up into seven distinct colors, each merging imperceptibly into the other, and ranged in the following order: red, orange, yellow, green, blue, indigo, and violet.

According to modern science the reason for this order of colors is found in the theory that luminous bodies impart to the ether waves of different lengths, and while passing through a prism the short waves are most retarded and the long ones least. Now, violet light is composed of short waves and red light of long ones. Therefore the violet is most retarded or bent, while the red pursues almost a straight course through the glass. The red and violet are consequently at each extreme of the spectrum, while the other colors arrange themselves according to their comparative wave lengths.

In 1802 Dr. Wollaston of England announced that the solar spectrum was not a continuous one, but was interrupted by a great many very fine, dark lines. Some years later (1812) a German physicist, Frauenhofer, investigated the character and positions of these remarkable lines and found them to be con-

stant and invariable in location. To the most prominent he applied the first eight letters of the alphabet, *A*, *B*, *C*, etc., and to-day we speak of Frauenhofer line *B* or *E*, or any particular one, to denote the region of the spectrum to which we wish to call attention. (*See* Fig. 1.)

The significance of these lines remained a secret until 1814, when Kirchhoff, in Heidelberg, made the startling announcement that they were due to the absorption of certain rays by the atmosphere surrounding the sun. He showed that if a metal be made incandescent and its light passed through a prism there would appear a number of bright lines in the spectrum, corresponding in position to some of the dark ones of the solar spectrum. Take, for example, the metal sodium or any of its compounds, and, placing a small quantity upon the end of a platinum wire, subject it to the flame of a spirit lamp; immediately the entire flame is yellow with the incandescent sodium vapor. Permit the light from this vapor to pass through a prism, and we will see a very fine, bright yellow line running vertically across the spectrum in the *yellow*.

Having noted the exact position of this line, remove the sodium flame and substitute a beam of sunlight. The beautiful full spectrum now appears, and just exactly where the yellow line of sodium existed is situated the dark Frauenhofer line *D*. Here, then, was a striking coincidence of location, and, regarding the phenomenon as something not to be overlooked, Kirchhoff at first theorized and subsequently proved the dark line at *D* in the solar spectrum and the bright line at *D* in the spectrum of incandescent sodium to be identical, due in each case to the presence of the vapor of the metal sodium. This is the way he arrived at the conclusion: he first saw that in the light of the sun the *D* line was *dark* while the glowing sodium gave a *bright* line. The thought occurred to him that the conditions were reversed, and he proceeded to experiment accordingly; and, without going too much into details, finally established the fact that *when the metal is itself luminous, or the source of light, the lines in*

the spectrum will be bright, but if the source of light be beyond the vapor of the metal the lines will be dark. In other words, if we volatilize sodium in *front* of a bright flame and cause the light from the flame to pass through the vapor before entering the prism the sodium line is *reversed*, it is dark. And so for other metals. Upon these experiments is based the present theory regarding *the constitution of the sun.*[1]

The delicacy of spectrum analysis in detecting metals varies with different ones. Of sodium $\frac{1}{200,000,000}$ of a grain will bring out the bright line at *D*. Of lithium $\frac{1}{60,000,000}$ and of calcium $\frac{1}{1,000,000}$ of a grain is sufficient to produce the characteristic effect.

It has been for some time observed that if the beam of light employed were permitted to pass through a colored fluid, before or after entering the prism, certain marked changes were invariably manifest in the spectrum. *First*, the most refrangible end (violet and blue) is entirely absorbed, and, *secondly*, one or more isolated dark bands of absorption are seen in the other colors, the *same coloring matter giving definite and invariable absorption.*

Very naturally physiologists caught the idea, and soon resorted to spectrum analysis in the cases of the colored animal fluids, blood, bile, and urine.

A brief description of the spectroscope and a few words concerning its technology are in place at this stage.

Spectroscopes are of various patterns and degrees of complication. A convenient form is made by Browning, of London. There is a narrow slit, regulated by a screw, through which the beam of light is admitted to the collimator tube. At the other end of this tube is a convex lens, which collects the rays and sends them parallel to the prism. Here they are

[1] That it is an intense mass of luminous matter in whose composition the metals play certainly an important part, and, being constantly volatilized by the great heat, are present in the atmosphere of the planet, and absorb certain rays according to the law established by Kirchhoff, *that metals in a gaseous state are opaque to those rays which they themselves are capable of emitting when incandescent.*

bent and broken up into the various colors, and are finally collected and magnified in the observing telescope. The observing telescope is arranged in such a manner as to allow of its rotation horizontally around the stand. This movement brings the different parts of the spectrum successively into view. The eye-piece is provided with two very small wires crossed in the centre of the field. These wires are a portion of the measuring apparatus. When the position of a line or band is to be ascertained, the point of decussation of the wires is placed exactly over the object in such a manner that the re-entering angles shall be equally divided by it. Reference is now made to a graduated scale upon the circular top of the stand and to a vernier scale sliding upon it and attached to the observing telescope. The position of the sun lines having been previously determined, it is easy to ascertain the location of absorption lines. Vierordt, a writer upon absorption spectra, proposes a very simple and convenient measuring method.[1] He considers the spaces between the fixed lines, *A*, *B*, *C*, etc., as divided into one hundred equal parts, and then indicates the position of an absorption band by the expression of its distance from any one of them. As *C* 50 *D*. But I fail to see the use of writing "*D*." So, with the exception of writing *the following letter*, this method will be employed in this paper.

I propose to speak of the fluids separately, and in the following order: *Blood*, *Bile*, *Urine*.

[1] *Die Anwendung des Spectralapparates zur Photometrie der Absorptionspectren.* Tübingen, 1873.

There are several other scales in use, but they require an additional tube than is possessed by the simple instrument already described. In this tube there is a scale photographed upon glass and so adjusted and illuminated that it is seen with the spectrum. This extra attachment involves greater expense, and I must say that I have found the method described in the text to be perfectly satisfactory for physiological work.

See "A New Method of Determining the Position of Absorption Bands," etc. By J. C. Dalton, M. D. Trans. New York Academy of Medicine, 1874.

"On a Definite Method of Qualitative Analysis of Animal and Vegetable Coloring Matters." By H. C. Sorby. Philosophical Magazine (4th Series), XXXIV.

BLOOD.

The coloring matter of the blood has been given a number of different names within the last ten years, which has created considerable confusion, particularly among those who are not especially interested in physiological chemistry, and have not followed the discussions on the subject. We see it called hæmatin, hæmatoin, hematosin, cruorine, hæmatocrystalin, hæmoglobulin and hæmoglobin. Berzelius suggested the term hæmatoglobin. Hemaglobin is employed by Hoppe-Seyler and the Germans, and will be used in this paper. Of some of the others we shall say more further on.

Hæmoglobin has the chemical composition $C_{54 \cdot 2}H_{7 \cdot 2}O_{21 \cdot 5}N_{16}Fe_{\cdot 42}S_{\cdot 7}$, and is a crystallizable substance. It was first obtained pure by Kölliker[1] in 1849 from the blood of the dog and fishes, and very soon afterward, 1851, by Funke[2] in the blood of man and the horse. The crystals are procured with variable success in different animals. The blood of dogs and guinea-pigs seems to yield them with greater certainty. They possess the same spectroscopic properties as a watery solution of blood, which is much easier to obtain.

Pure defibrinated blood is perfectly opaque in any appreciable volume, and is not suitable for spectroscopic work. But, when a small quantity of water is added, the corpuscles are deprived of their coloring matter, which is perfectly soluble in water, and what is left of the albuminous corpuscle is practically transparent; so that we now have a tolerably pure solution of hæmoglobin more or less translucent according to the degree of dilution.

Suppose we take a cubic centimetre of pure defibrinated ox-blood, and, diluting it with an equal volume of water, place it before the slit of the spectroscope. We shall find everything dark with the exception of a narrow band of bright red. While the eye is still at the instrument add, very gradually, more water, and observe the remarkable change. By degrees

[1] "Microscopical Anatomy." Philadelphia Ed., 1854, p. 714.
[2] "De Sanguine Venæ Lienalis." Lipstadt, 1851.

the red band grows wider, then the *orange* appears. A very slight additional dilution will permit the *green* rays to pass, and now the appearance is that represented in Fig. 3 of the chart. The *red*, *orange*, and a portion of the *yellow* showing at one extremity of the spectrum, and a dark, broad band, with its edges well defined, separated from the darkness of the other extremity by a band of *green* light. The position of this band would be indicated by saying that it commenced at about *D* and extended to about *E* 10. (The position of all absorption bands is more or less affected by concentration of the solution and the thickness of the layer through which the light has to pass.) This, then, is the spectrum of what we should call a pretty concentrated mixture of blood.

Proceeding now to dilute with water, very carefully, another change will take place. Gradually the light appears in the rest of the *green* and the beginning of *blue*. But note the singular operations at work in the broad dark band. It has been divided by a strip of yellowish green and now we have two absorption bands. The first narrow, dark with sharp cut edges situated at the right of *D*; the second broader, its borders blurred and placed just on the left of *E*. *This is the characteristic spectrum of oxygenated hæmoglobin*, that to which Hoppe-Seyler first called attention.[1] (*See* Figure 4, chart.)

It is possible to carry the dilution to a surprising extent before these bands disappear. As water is further added the entire spectrum gradually clears up, interrupted only by the very indistinct and shadowy remains of the two original bands. And it is interesting and important to observe the greater persistence of the first, for when its mate has quite vanished it is still to be seen.

Hoppe-Seyler thought that these phenomena would be of great assistance in ascertaining the nature of the coloring matter and enable him perhaps to follow it through its decompositions to the bile and urine, the color of which fluids have been thought to be due to altered hæmoglobin.

[1] "Virchow's Archiv," xxiii., 1862, p. 446.

He found that alkaline carbonates and caustic ammonia did not affect the spectrum, that is, did not alter the hæmoglobin, but that acids and fixed caustic alkalies decompose it with an accompanying change in the spectrum; but more of this hereafter.

Prof. Stokes,[1] of England, attracted by Hoppe-Seyler's paper, pursued another path of enquiry.

So much having been said about the difference in color between arterial and venous blood being due in the one case to the presence of oxygen and in the other to its absence, he sought the aid of the spectroscope in the matter and performed the following experiments:

Taking a solution of ox-blood which showed the two bands, (Fig. 4) he desired to find out whether they were in any way due to the presence or absence of oxygen. He therefore added reducing agents which would appropriate any free oxygen in the solution. Ferrous sulphate, to which a little tartaric acid had been added and then made alkaline with ammonia, was generally employed. (The agent was to be used with an alkaline fluid and the tartaric acid is put in to prevent a precipitation of ferrous carbonate.) When a little of such a deoxidizing substance is added to our blood solution the first thing noticed is a darkening of color, recalling the hue of venous blood. But for us the most important change has occurred in the spectrum. Placing the altered blood before the slit of the spectroscope we are confronted with a remarkable change. Our pair of distinct bands have disappeared, and a single, dim, broad band is seen commencing at about *D* 10 and extending to *D* 90. The spectrum has also become visible as far as *G*. (Fig. 5, chart.)

Stokes reasoned that this must be the spectrum of reduced blood. The proof was simple. Shake the solution with air and ascertain whether the single band will give place to the original two. It does, and the reduction and oxygenation may be repeated until the color of the reagent begins to interfere with that of the blood. It was ascertained that these

[1] Proceedings Royal Society, vol. xiii., 1864.

changes were not due to the color of the reagent employed, as others perfectly colorless gave the same result.

Stokes also found that, if a full bottle of oxygenated blood solution be tightly stopped and set aside for about twenty-four hours, at the end of that time the color will have changed to that of our reduced hæmoglobin, and the same change in the spectrum will have been effected. Agitation with air will restore the red color and the bands of oxygenated hæmoglobin.

These phenomena are very interesting, and are indeed, as proved by Prof. Stokes, the analogues of what takes place in the blood in the living body. The aërated hæmoglobin represents arterial blood, and reduced hæmoglobin venous.

Stokes's conclusion is "that the coloring matter of the blood is capable of existing in two states of oxidation, distinguishable by a difference in color and a fundamental difference in their action on the spectrum. It may be made to pass from the more or less oxygenated state by suitable reducing agents, and recover its oxygen again from the air."

But it is important to mention here that venous blood as it exists in the circulation does not give the spectrum of venous hæmoglobin. Dr. Sharpey, of London, has demonstrated this fact, and we have repeated his experiments. In order to demonstrate this it is of course necessary to render the blood transparent; but the addition of water is equivalent to aëra tion. Therefore we must employ water from which all the air has been expelled by boiling in vacuo.

A glass syringe having been filled with this water the nozzle is inserted into a vein and a few drops of blood drawn up. The solution thus contained in the glass cylinder is placed before the spectroscope for examination. And as before stated venous blood under normal conditions gives the two bands of oxygenated hæmoglobin. This is only another of the wise arrangements of Nature where she provides against sudden arrests of respiration proving instantaneously fatal. These experiments prove that there is sufficient oxygen in venous blood to bear another circuit or so and to meet the demands of nutrition. But suppose oxygen is prevented entering the lungs for

a longer time, or is permanently prevented or is displaced from the blood by other gases, what should be the condition of venous and even arterial blood regarding oxidation then? Under such influences it has been found to be completely deoxidized and to correspond spectroscopically with Stokes's reduced hæmoglobin. (Except in case of displacement by some gases, where we have other characteristic spectra.) Of course after the residual air has been exhausted from the lungs the arterial blood will not differ from the venous. In fact all of these phenomena may be studied upon the living animal with the circulating blood. We have simply to employ a transparent living membrane, such as the web of the frog's foot, in the place of our blood solution. Then, although the picture is somewhat indistinct and mottled, on account of the interruption of the light by the tissues, the hæmoglobin bands are easily discerned, and, what is still more important, give place to the single reduced band if respiration is arrested.

A. Schmidt[1] has found that destruction of the medulla oblongata and death by blows upon the head will enable one to see the broad band of reduced hæmoglobin in the blood of the animal. These are only modes of suspending respiration while the circulation is still maintained.

It is evident, then, that the blood in passing through the lungs is oxygenized and afterwards loses this oxygen. How and where does this latter change take place?

We have already seen that it will occur in a solution of blood which is simply allowed to remain at rest protected from the air, and Schmidt, in the course of experiments just alluded to, with a view of determining the reducing power of different tissues upon blood, gives the following results:

If he exposed a muscle of a frog in blood outside of the body and produced continuous tetanus, the oxygen was disassociated quicker than by muscle at rest, but not so rapidly as by a dead muscle. Brain and liver substance reduced a watery solution very rapidly, the solution being at 0° C. The phenomena presented by the full bottle show that the blood

[1] "Centralblatt," 146.

itself contains substances, the nutritious matters, which have a great affinity for oxygen but are not capable of appropriating it so rapidly as the coloring matter, and must needs be in longer contact with it in order to be oxidized. And the experiments of Schmidt demonstrate that the tissues are capable of producing the same change. In the hæmoglobin oxygen is in a state of *physical admixture*, in the other ingredients of the blood and in the tissues it is *chemically combined.*

That oxygen is very loosely combined with hæmoglobin is shown by the displacing power of various gases upon oxygenated blood, and by the results of submitting it to the air pump.[1]

SPECTROSCOPIC EFFECTS OF GASES AND REAGENTS UPON BLOOD.

Carbonic Oxide, CO.—When a stream of this gas is passed through aërated blood the oxygen is displaced and the carbonic oxide unites with the coloring matter in equal volume.

Before the spectroscope carbonic oxide blood shows very interesting peculiarities. We have two about equally wide and dark bands situated in the yellow between *D* and *E*. To the unpractised eye they might seem identical with the bands of oxy-hæmoglobin. But these latter do not bear so close a resemblance to each other as those now under consideration, and are situated farther toward the blue.

The distinguishing feature of the carbonic oxide bands, however, is to be found in their behavior with both reducing and oxidizing agents.

Neither of these affect them. But, if the solution be allowed to stand for several days at an ordinary temperature, these bands disappear and the broad band of reduced hæmoglobin is substituted.

Hoppe-Seyler first showed that blood in combination with carbonic oxide would not be affected by reducing agents, and Dr. Gamgee[2] found that carbonic oxide would produce the

[1] It has been found that, when blood is subjected to the air-pump, its coloring matter will be deprived of its oxygen when the pressure falls to about twenty-five millimetres of mercury.

[2] "Medical Times and Gazette," 1866, p. 325.

same bands after reduction had been accomplished and even in presence of an excess of the reducing agent. He further pointed out that poisoning by carbonic oxide or charcoal fumes invariably produces carbonic oxide blood.

Nitrogen Monoxide, N_2O (Laughing-gas).—The effect of this gas upon blood is to produce an intense red color. There is, however, no change in the spectrum, and reducing agents will cause the broad band of Stokes to appear.

Carbonic Acid Gas, CO_2, simply displaces the oxygen and shows the spectrum of reduced blood.

Poisons.

Preyer has experimented upon blood with a great variety of reagents, with most of which he obtained characteristic spectra. It is only necessary to mention a few to show that we have a valuable aid in the spectroscope in detecting these agents. (Harley[1] also, it may be mentioned, has published a detailed account of researches in this direction, but with no particular reference to the spectroscope.)

Nitrites.—We are indebted to Gamgee[2] for very interesting reports of the action of this class of substances upon the blood. He found that "all nitrites except that of silver show some crystalline form, color, and spectrum." The blood assumes a chocolate hue under their influence, the two oxy-hæmoglobin bands becoming very faint and another appearing at *C*. Ammonia causes the two bands to grow more distinct, and two entirely new ones (making three) to show themselves in the orange, the blood at the same time resuming its red color.

Oxidizing and reducing agents act perfectly with nitrite-blood. The reducing agent first restores the normal oxy-hæmoglobin spectrum and then induces that of deoxidized; seeming to show that the nitrate superoxygenated the blood.

Nitrites so affect blood as to enable it to resist the action of carbonic oxide. But the loose oxygen is not destroyed or dis-

[1] "Proceedings of the Royal Society," vol. xiii., p. 157.

[2] "Philosophical Transactions," 1868, p. 589.

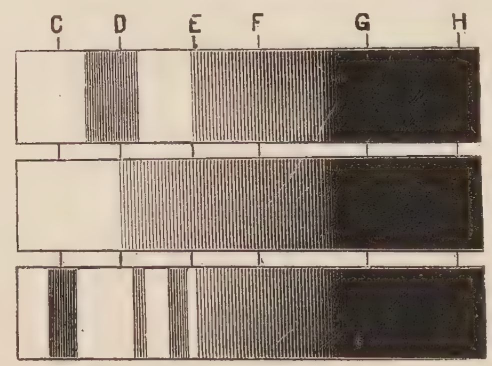
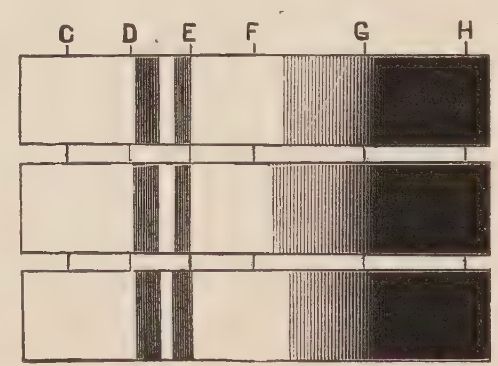
pelled; it appears to be "simply affected in an unknown manner." Gamgee founds this statement upon the behavior of nitrite-blood with reducing agents; as we have just seen, they first restore the spectrum of oxy-hæmoglobin before inducing the reduction, without the aid of atmospheric air.

Quantitative Spectroscopic Analysis of Hæmoglobin.—We have seen that blood gradually diluted before the slit of the spectroscope permits first the red and next the green light to pass (Figs. 2 and 3). It has been found by Preyer,[1] to whom we owe so much in this department of physiological chemistry, that the degree of dilution which allows the green to show itself is so constant that a sufficiently accurate method of quantitative analysis of hæmoglobin can be based upon it. The process is as follows:

A concentrated solution of a known weight of hæmoglobin crystals is placed in a glass chamber with parallel sides which are one centimetre apart. This chamber or "hæmatinometer" is now placed before the slit of the instrument, and the light allowed to pass through. Then proceed very carefully to add distilled water from a finely graduated burette, and stop the moment green light begins to show. Observe the width of the slit and the position of the light. The solution now contains a percentage of hæmoglobin, which will always be the same under the same conditions. Let K represent this percentage.

Now, if we wish to estimate the percentage of coloring matter in a specimen of blood, we have only to repeat the above process. First, be sure that the conditions as to the light and width of slit are the same. Then take a measured quantity of the blood, agitate it well with air, and dilute it with a small measured volume of water. This blood solution is then placed in the "hæmatinometer" before the slit and further diluted with water from the graduated burette until the green begins to come through. Now we have arrived at the point reached in the first experiment. A little mathematical calculation only remains to ascertain the result. $k =$ the

[1] "Die Blutkrystalle," Jena, 1871, p. 121.

SOLUTION OF BLOOD.	REAGENT.	OXYGENATED.	REDUCED.	REMARKS.
	Caus. Potass......			Can be alternated.
	" Ammo................			" "
	Alk. Carb....................			Cannot be alternated.

HÆMATIN.

percentage of hæmoglobin present under the given conditions. Let the total quantity of water added $= w$, and the amount of blood employed $= b$. Then we shall have the equation:

$$x = k\frac{(w+b)}{b}$$

Decomposition Products of Hæmoglobin.

The coloring matter of the blood is subject to spontaneous decomposition, in the body and out of it. These decomposition products can also be produced by treating hæmoglobin or blood with certain reagents.

Those best known and which give definite spectra are: 1, Hæmatin; 2, Hæmatoin; 3, Methæmoglobin; and 4, Hæmatoidin. We shall study them in the order enumerated.

Hæmatin.—$C_{96}H_{102}O_{18}N_{12}Fe_3$.

Caustic Potassa.—If a solution of hæmoglobin or blood be treated with caustic potassa the color becomes brownish and in its spectrum we have a dim broad absorption band commencing at C 50 and extending to D 40 (*see* Fig. 6, chart).

This is hæmatin. It is an important fact that this substance is capable of oxidation and reduction. The spectrum just described is that of oxygenated hæmatin. Add a small quantity of the reducing agent, and, once more examining the spectrum, observe what has taken place. The shadowy broad band has gone and the *green* and *blue* rays are much less obstructed. The most striking change, however, is the appearance of two entirely new bands, the first situated at D 30, and the second on the left of E. Both are very black and remind us at once of the bands of oxygenated hæmoglobin. But a little attention will show that they are placed further to the right, and another point of distinction is that the first band of reduced hæmatin is the broadest and appears distinct before the second has begun to show itself (Fig. 7, chart). By agitating the reduced solution with atmospheric air these bands will disappear and the single one of oxy-hæmatin will return. Hæmatin produced by this means was called by Stokes "alkaline hæmatin" to distinguish it from the products by acids.

But it has been shown by Preyer that the "acid hæmatin" of Stokes is not hæmatin, but an entirely new substance which he calls *Hœmatoin*.

Caustic Ammonia.—Hoppe-Seyler describes hæmoglobin as unaffected by this reagent, but we have found it capable of producing hæmatin. It is true that the spectrum is not very decided in the oxygenated state, but reducing agents render the proof. We have found that a solution of blood treated with caustic ammonia placed before the slit of the spectroscope will not give any well-marked band, but simply absorption of the refrangible end of the spectrum up to *E*. Treated with a reducing solution the two black, sharp-cut bands of reduced hæmatin immediately appear, and what is important they can be banished by agitation and made to reappear at pleasure by again reducing them.

Alkaline Carbonates.—Regarding the action of these substances we must again differ from Hoppe-Seyler, and agree with Burdon Sanderson[1] and Stokes that they determine the production of hæmatin. Their action is not so rapid, requiring a day or two, nor is the first spectrum like that of either of the preceding. The reduced spectrum is, however, the same.

We treated a solution of sheep's blood with carbonate of soda, and examined it after four days, kept at ordinary temperature. It then gave the bands of oxy-hæmoglobin quite indistinctly and another well marked in the orange on the right of *C*. On the application of deoxodizing agents all of these vanished, and the unmistakable pair of reduced hæmatin once more came boldly in view.

I was not able to restore the band near *C* by agitation with air. Those of oxy-hæmoglobin, however, returned more distinct than before. It must have been that the transformation into hæmatin was not complete and the subsequent manipulation so affected it as to render its spectrum invisible.

This completes the description of the methods which have produced hæmatin in our experiments.

[1] "Hand-book for the Physiological Laboratory." American edition, p. 197.

Other reagents are mentioned by authors as determining the formation of this substance. Stokes gives in addition, acetic and tartaric acids, alcohol, and heat. Dr. Waterman[1] mentions chloroform. They do not claim for these the same spectroscopic features, but say that the position and number of bands depend upon the agent used.

Thus by acetic acid Stokes describes "four banded acid hæmatin," and by a caustic alkali one band "alkaline hæmatin." Now, the "acid hæmatin" was not affected by reducing agents, but, being a product of decomposition of the coloring matter of the blood, he called it hæmatin. We have classed it under Hæmatoin.

Therefore, instead of calling the results of the action of all the above agents *hæmatin*, it appears a better classification to recognize as this substance only those which by deoxidation show the *two bands of reduced hæmatin*, without special regard to their spectral agreement in the oxidized state where, according to my observations, they differ with each reagent. The following table will perhaps be useful. It illustrates the three varieties described in this paper.

It will be seen, from the foregoing table, that the spectrum of hæmatin formed from an alkaline carbonate is identical with the spectrum of methæmoglobin (Fig. 7, chart). But there is no danger of confounding them when we treat each with a reducing agent. The methæmoglobin is unaffected, while the other undergoes the change already spoken of.

Hæmatoin.—When either acetic or tartaric acid is added to a solution of blood, or its crystals, in large quantity, the formation of hæmatoin (Preyer), acid hæmatin (Stokes), iron-free hæmatin (Hoppe-Seyler), is determined.

The most certain method of obtaining the spectrum of this substance is that advised by Stokes. Take a tolerably concentrated blood solution and to it add equal volumes of acetic acid and ether. Mix well but not violently, for an emulsion will form. Allow the ethereal extract to collect at the top, and, having carefully poured it off, subject it to spectroscopic

[1] *Medical Record*, New York, October 15, 1874, p. 532.

examination. There will four bands appear (Fig. 8, chart), one broad and plain at *C*, one narrow and very dim at *D*, a broad and well-marked one at *D* 75, and the last, broad, with shadowy outlines, at *E* 70.

We have not always succeeded in seeing the *D* band, and in order to do so the solutions must be varied regarding concentration, and the light delicately regulated at the slit.

I think the effect of tartaric acid alone is sometimes as satisfactory as the ether solution, and would especially recommend it to reveal the band at *D*.

Hæmatoin is not capable, like the foregoing substances, of existing in two states of oxidation; reducing agents do not affect it, nor does agitation with air.

METHÆMOGLOBIN.—This is a substance found in old extravasations, hydrocele and ovarian fluids, and is a product o the decomposition of hæmoglobin. It was first described by Hoppe-Seyler and named by him. The composition of this substance is not very well understood. It can be prepared in several ways. If a dilute solution of blood is kept for several days at ordinary temperature, or if very *small quantities* of any acid are added to a more concentrated specimen, methæmoglobin will form. These methods amount to the same thing in the end, for in stale blood there are acids developed, formic and butyric, which give a distinct reaction to test paper.

Examined with the spectroscope it shows, in addition to the characteristic blood bands (Fig. 4, chart), another dark band at *C*. Treated with reducing agents these all disappear with the production of the single band of reduced hæmoglobin (Fig. 5, chart). This is because the methæmoglobin band *C* depends upon an acid condition, and our reducing agent being alkaline destroys it, and sufficient hæmoglobin is present to respond in its well-known manner.

Give the hæmoglobin time to decompose and we shall have pure methæmoglobin, which, according to Preyer,[1] gives a spectrum similar to hæmatoin. It would seem, then, that methæmoglobin is simply partially-formed hæmatoin.

[1] "Die Blutkrystalle," Jena, 1871, p. 191.

Considerable care is necessary in producing this substance with acetic and tartaric acids. A few drops of a dilute solution are sufficient to bring the *C* band into view. If a greater quantity is added the blood bands gradually fade, the spectrum at the same time clearing up in *green*, *blue*, and *violet*, and shortly, as in the case where hæmoglobin is allowed to entirely decompose, the four bands of hæmatoin are given. A concentrated solution of blood should be taken to begin with, and never add enough of the reagent to destroy the hæmoglobin bands.

In preparing methæmoglobin by adding acids, the position of the absorption in the orange varies according to their strength. The stronger the acid the farther toward *B* will the band be situated. By using stale blood and adding an excess of acetic acid the band near *C* does not shift, but contracts upon both sides (Preyer). We have found nitric and sulphuric acids to be equally capable of giving the spectroscopic characters of methæmoglobin.

HÆMATOIDIN.—This is another of the decomposition products of the normal blood color first described by Everard Home in 1830, and subsequently brought to light again by Virchow[1] in 1847. It is crystallizable and its crystals are easily discovered in extravasated blood of the corpora lutea, apoplexy, sputa of pneumonia, and in obliterated veins.

Hoppe-Seyler[2] states that this substance is identical with *bilirubin*. But Preyer[3] asserts that they are distinct, in that their spectra are unlike. This difference can be seen when a chloroform extract of gall-stones is made. We then have a bright yellow fluid, which before the spectroscope gives no well defined absorption bands either in dilute or concentrated solution (bilirubin). On the other hand, a chloroform solution of an apoplectic clot or corpora lutea of the cow gives us a yellow fluid, which exhibits distinct and characteristic bands

[1] "Archiv für pathologische Anatomie," u. s. w., Band I., § 383.

[2] "Handbuch der Physiologisch- und Pathologisch-chemischen Analyse," Berlin, 1870, p. 178.

[3] "Die Blutkrystalle," Jena, 1871.

(Fig. 10, chart). Very intense illumination is necessary to show this spectrum.

Leutine.—This is the yellow coloring principle which is generally said to exist in the yellow bodies of the ovary, in the yolk of eggs, and, according to Thudichum, in certain vegetables.

It is soluble in alcohol, chloroform, and ether, and its solution presents a decidedly yellow color.

Some confusion exists as to the indentity of leutine, bilirubin, and hæmatoidin. A chloroform extract of the cow's ovary we have seen gives the spectrum of hæmatoidin. We have failed to obtain any satisfactory results in the examination of ovary extracts and fluids from ovarian tumors. Preyer took an ovary in which hæmatoidin crystals were very abundant, and made a chloroform extract of it. Before the spectroscope only the bands of hæmatoidin were seen. But a similar extract of the yolk of egg gave an entirely different spectrum; so this observer holds that the corpora lutea contain no leutine, but hæmatoidin. As regards bilirubin we have seen that it gives no definite bands, and therefore there is no danger of confounding it with either of the above. The characteristic spectrum of leutine is furnished by the extract of the yolk of egg (Fig. 12).

Myochrome.—This is the coloring matter of red muscular tissue, and can be extracted by maceration in water. We shall then have a yellowish fluid, which before the spectroscope presents all the characters of hæmoglobin; carbonic oxide and other reagents affecting both alike.

Bile.

The color of the bile varies in different animals, and sometimes in the same animal, from a green to a reddish brown. This great diversity is due to the presence of two entirely distinct coloring matters, *bilirubin* and *biliverdin*. The intermediate tints would naturally depend upon the relative increase of either of these pigments and produce shades of green and red accordingly. It is on this account that ox bile

may be perfectly green, olive, olive-brown and reddish-brown. Other animals do not furnish quite so much variety in color. Human bile is more or less of a golden-brown, sheep's bile is green, pigs' bile is reddish-brown, rabbits' bile olive-green, dogs' bile golden-bronze, etc. It was at one time supposed that biliverdin and the green coloring matters of plants were identical, but Prof. Stokes [1] proved by spectrum analysis that they are not.

It is a favorite and fascinating idea among some physiologists that these bile-coloring matters are derived from the blood; that they are products of the decomposition of hæmoglobin. Experiments on this subject disagree in a remarkable manner.

The most recent and elaborate were performed by Steiner, in Berlin, and Tarchanoff, of St. Petersburg, who worked in Hoppe-Seyler's laboratory.

The object of the experiments was to determine whether, by injecting water and solutions of blood into the circulation, bile pigment would show itself in the urine. Injections of water were intended to dissolve the corpuscles and set free the coloring matter; blood solutions to supply an excess of the same; in either case to furnish favorable conditions for transformation.

Steiner, in twelve cases with rabbits, injected 10 c. c. of water at 100° Fahr. into the carotid arteries—two of these showed bile color in the urine. In an equal number of injections into the veins no bile color appeared in the urine, nor did the blood color come through the kidneys. The result of six experiments with 20 c. c. of water was negative. But, when from 30–50 c. c. were injected, bloody urine resulted, and *post-mortem* examination revealed bloody serum in all the cavities, *but no bile color*.

Tarchanoff thinks that Steiner did not take sufficient precautions, and publishes the result of his own investigations.[2] He used dogs, considering the slimy urine of rabbits not suit-

[1] "Proceedings Royal Society," 1864, xiii., p. 144.

[2] "Archiv für Anatomie und Physiologie," 1873, ii., p. 160.

able. He also guarded very carefully against foreign matter by inserting canulæ directly into the ureters, from which to collect the urine.

The animals were freely fed with meat before the operation. A pure solution of hæmoglobin was prepared and injected into the circulation, followed by very evident bile color in the urine. Injected 150 c. c. of water with a similar result. Steiner, he thinks, must have therefore overlooked these results in his own experiments, or what is very probable hares and dogs give different results.

This, then, is a glimpse at the literature of the subject.

It was thought that the spectroscope might throw some light upon the question. But unfortunately those who thus examined the bile said that it gave no definite spectrum.

A paper on the "Spectrum of Bile," by Prof. Dalton,[1] in 1874, however, gives a new impetus to the subject; for he has discovered that there is a reliable character in the bile spectrum, which we ourselves, having repeated his experiments, can testify to and shall proceed to describe.

Prof. Dalton first describes the visible spectrum of all kinds of bile as being very short and terminating very suddenly at or about *E* 45. He next states that he has found a dark, well-marked band at *C*. He says: "According to my own observation it is so constant and so well marked as to form a characteristic feature in the spectrum of bile whenever it has a decided greenish tint, and often when it is of a yellowish, reddish, or olive-brown color. In eleven specimens of ox bile, sheep's bile, and dogs' bile, of a green, greenish-olive, olive, or olive-brown color, this band was visible when examined in a thickness of only two or three centimetres. In all the nineteen specimens of ox and sheep's bile which had a greenish or olive tint, when viewed in a thickness of two or three centimetres, the band at *C* was very distinct and often quite dark or almost black. In the three remaining specimens which were of a yellowish-brown or olive-brown color, it was distinct and sometimes very dark in layers of two centimetres;

[1] *New York Medical Journal*, June, 1874.

and in a sixth it was perceptible in layers of three centimetres."

We have examined nine specimens of ox bile, six of sheep, one of dogs, and one of rabbits.

A detailed account is as follows:

Ox bile.—Specimen No. 1. Brownish-red. Removed from the body one hour. Viewed in thickness of from 2–5 centimetres, showed indistinct band at *C*. Spectrum ends at *E*.

Specimen No. 2. Olive-green. One hour removed from body. Thickness of from 2–3 centimetres, very distinct band at *C*.

Specimen No. 3. Dark green. One hour after removal. Thickness of centimetre reveals exceedingly dark band at *C*. Spectrum stops at *E*.

Specimen No. 4. Dark green. One and a half hour after removal. Two centimetres thick gives well-marked band at *C*. Spectrum terminates at *E* 50. This specimen is so transparent that the most satisfactory thickness to employ is five centimetres, when the *C* band is absolutely black and sharp cut at its edges.

Specimen No. 5. Olive-green. One and three-quarters hour after removal. *C* band visible in layers of from 1–3 centimetres.

Specimen No. 6. Olive-brown. One hour after removal. Shows no sign of the band at *C*. Spectrum ends at *E*.

Specimen No. 7. Olive-green. One hour after removal. In thickness of five centimetres. The *C* band is distinct. Spectrum ends at *E*.

Specimen No. 8. Olive-green. One hour after removal. In layer of five centimetres, shows *C* band very plain. Spectrum ends at *E* 30.

Specimen No. 9. Olive-green. One hour after removal. In layer of four centimetres, shows band at *C* very distinctly.

Sheep's bile.—The six specimens were all similar in color (dark green) and were removed from the body one hour before

examination. In thickness of from $1\frac{1}{2}$–3 centimetres the band at *C* was in every case very distinct.

Dog's bile.—Golden-bronze color. Forty minutes after removal. Layer of one centimetre showed *C* band very dark and well marked at its edges.

Rabbit's bile.—Dark green with slight olive tint. Four hours after removal from the body. Layer of one centimetre reveals *C* band. These results added to those of Prof. Dalton cause us to wonder at the negative results arrived at by previous observers.

The only specimens in which the *C* band seems to be ever absent are those which have a more or less brown or reddish tint. Never in green bile have I failed to see this band (*see* Fig. 12, chart,) and it appears from those facts that the band at *C* is characteristic of the green element in bile. This is shown experimentally as follows: Add nitric acid or tincture of iodine to a specimen of yellow bile which does not show the *C* band. (These agents oxidize the solution with the effect of rendering the color green.) This specimen will now give the *C* band. The green color is due to oxidation, and the power of oxidizing substances to strike a green color with bile has been long known; but Dalton has found that if a full and tightly-stoppered bottle of green bile, which shows the band at *C*, be set aside for twenty-four hours, the green color will fade and give place to an olive or brown, and will then fail to reveal the *C* band; subsequent exposure to the air or addition of oxidizing agents will restore the green color and its absorption band. This behavior is analogous to that of blood when treated in the same way.

Bile also shows several other bands, but which are not constant or well defined in every instance. These bands seem to have nothing in common with the *C* band. That is they appear sometimes in green and sometimes in brown bile. The most constant are a faint, broad one on the left and a dark, narrow one on the right of *D*.

Does the fact that bile possesses a characteristic spectrum, then, throw any light upon the question as to the origin of its

pigment? Are we able to trace it to any of the products of hæmoglobin? As yet we must answer in the negative—we must await the result of investigations based upon Prof. Dalton's discovery.

Pettenkofer's Test for Biliary Salts.

As is well known, this is a color test. It depends upon the solution changing to a fine purple. But it has been found that there are other substances which, by treating with cane sugar and sulphuric acid, will present the same color. Such are various albuminous bodies, the salts of opium, theine, ethereal oil, cerebric acid (Thudichum), amyl alcohol, etc. But the spectroscope will decide whether the colored reaction be due to either of these or to the bile salts.

The purple reaction characteristic of Pettenkofer's test is usually too concentrated even in very thin layers for spectroscopic analysis. Water, added with a view to dilute it, only renders it still more opaque by precipitating cholic acid. But it has been found by Prof. Dalton[1] that, if a very dilute watery solution of the sodium glycocholate (1–500) be taken to begin with, subsequent dilution with water will not cause a precipitate. Therefore it is well to use very dilute solutions of the suspected substances if they are to be placed before the spectroscope. (Should it be necessary, sulphuric acid may be used to effect the dilution.)

Pettenkofer's test in watery solution shows one broad and distinct band at *E*, the refrangible end of the spectrum being invisible up to *G*. In alcoholic solution the same band at *E* is seen with another not so broad at *F*. Egg albumen treated with Pettenkofer's test shows a broad, shadowy band commencing at about *D* 60, and extending to *F*. The spectra of codeine and morphine are the most like those of the bile-salts, showing a band at *E*; but this band is not near so well defined, and disappears suddenly on the addition of a small quantity of water, while in the case of the latter a considerable dilution is necessary to destroy it.

[1] "Human Physiology," Philadelphia, 1875, p. 214.

Urine.

It was expected that the spectroscope would reveal some positive information as to the origin of the coloring matter of the urine. But it is found that this fluid does not yield any satisfactory results by prismatic analysis. Ordinary urine shows no absorption bands at all, and a specimen which I evaporated from 30 ℥ to 5 ℥, thereby causing it to present a very dark, reddish color, still gave a negative result.

But by adding nitric acid and allowing to stand I have once succeeded in getting a broad, indistinct band at *F*. This band has been seen by other observers, and, because the urine sometimes shows it, there is a theory that urochrome is derived from the bile colors, in that by extracting dog's bile with hydrochloric acid we obtain a substance which gives a very similar band. Further treatment of urine and the bile-extract brings about other spectral changes, which give strength to the theory just stated (Sanderson).

Coming now to inquire into the practical worth of spectroscopic analysis, we are reminded that there is not such a distinction to be made between the two terms "scientific" and "practical" as is generally accepted. The fact is, we never know how very practical a very scientific discovery may at any moment become. It was a scientific course of experiments which preceded the practical use of electricity; the investigations of Goodyear and others into the properties of india-rubber were strictly scientific, and led to the most practical results. And so of almost every useful invention.

Now, physiological spectroscopy is in its infancy, and we may reasonably look for important practical results within the near future. Let us see what it has already accomplished. Stokes's experiments have materially added to our knowledge of the respiratory processes, showing conclusively that the function of the red coloring matter of the blood-corpuscles is to absorb, convey, and deliver oxygen; that the difference in hue between venous and arterial blood is due simply in the

one case to the absence, and in the other to the presence of oxygen.

But it is in the field of forensic medicine that we shall find the spectroscope of absolute use. The detection of blood-stains by the microscope depends upon the presence of the corpuscles. But these elements are very easily dissolved, or otherwise rendered unnatural. Here the microscope would fail; but the spectroscope will reveal reliable signs, as we have seen that even the decomposition products of hæmoglobin give characteristic spectra.

Specimens of blood, wet or dry, kept for long periods undergo changes which render the microscope useless as a means of detection. But, spectroscopically, the presence of blood can be absolutely proved.

For instance, dried blood (on rags, wood, glass, etc.) kept for some days turns brown. It is now practically insoluble in water, and, in order to subject it to examination, we must dissolve in a weak acid. This at once destroys the corpuscles, and the microscope is valueless. But by prismatic analysis we see the two hæmoglobin bands, and in addition one at *C*. It is either methæmoglobin or hæmatoin. Treatment with reducing agents will determine which, and absolutely prove the presence of blood. The broad band of reduced hæmoglobin will appear, if it is the first; if the second, they will have no effect.

Letheby and Sorby examined stains on linen seventeen years old, and the spectra were in every respect characteristic.

We have a specimen of pure sheep's blood which has been kept at ordinary temperature more than a year. The corpuscles have disappeared, it is swarming with bacteria, and the microscope can furnish no proof that it is blood, examined as it stands. Yet it shows the absorption bands of hæmoglobin, and reacts perfectly well with deoxidizing substances. Benoit[1] speaks of a similar observation. Sorby kept dried blood (on rags) exposed to the air, and found that it was converted into methæmoglobin, with different degrees of rapidity, according

[1] "Études spectroscopiques sur le Sang," 1869, p. 60.

to the locality. In towns and in inhabited rooms the change took place sooner than in the country well removed from houses. He also found that dried stains sealed in tubes required three months to change; if sealed wet, they did not change at all. Cited by Letheby.[1]

Some vegetable and animal coloring matters may confuse us at first, but the application of oxidizing and reducing agents will give conclusive evidence of the presence of blood.

Substances most likely to be confounded with blood-color are, an ammoniacal solution of carmine, and solutions of iron sulphocyanide. These, when placed before the spectroscope, show such different absorption as to enable one readily to distinguish them from blood and from each other. An important test for vegetable colors is ammonia. I believe there is not an instance where this reagent does not markedly change the hue, while it has little or no effect upon blood.

Regarding the question, Does prismatic analysis enable us to distinguish human blood from that of the lower animals? the answer is, It does not. The spectrum is alike in all red blood. (Hoppe-Seyler, Valentine, Bert, Benoit.)

The presence of other coloring matters does not interfere with the detection of blood. I have found that one drop of blood added to an ounce of green ox-bile gives its characteristic absorption very distinctly.

It is sufficient to call attention to what has been said of the action of gases and poisons upon blood to show how important the spectroscope would be in cases of death from any of those substances. With some of them it would be the only means of determining the cause of death, while with others it certainly would greatly strengthen the evidence adduced. The delicacy of this method of examination is very great in the case of blood.

Sorby states that $\frac{1}{100}$ of a grain could be made to show all the transformations, and he says that by a little practice $\frac{1}{500}$ or $\frac{1}{1000}$ of a grain can be detected.

It is further said that with the Sorby-Browning micro-

[1] "Spectrum Analysis." Reports, London Hospitals and Clinics, 1866.

spectroscope a single blood-corpuscle will show the characteristic absorption. Indeed, the first recorded case of the use of the micro-spectroscope in a murder case is where the suspected criminal was near being acquitted on account of the failure to establish the proofs that there were any evidences of blood upon a hatchet which was found in the woods near the victim, and with which, from the character of the wounds, it appeared he must have been attacked. The implement had lain exposed to the weather several weeks, and the most careful examination failed to discover the slightest traces of blood until it occurred to the expert to remove the handle. Then, upon the end which was inserted into the iron ring, a dark, reddish stain was seen. Careful sections of the wood were made, the stain dissolved in a few drops of acidulated water, and, with a $\frac{1}{25}$ inch objective on the micro-spectroscope, this fluid showed the indisputable blood bands, and the prisoner was convicted.[1] Herapath[2] estimates that there was about $\frac{1}{1000}$ grain of blood in the portion examined.

In conclusion, let us say that this is a strikingly beautiful means of investigation; it is free from the objections which chemical analyses of the coloring matters offer, viz., numerous apparatus and reagents. With coloring matters other than blood, I think its chief value, however, at present is in verifying other analytical methods. But we hope by improved illumination and construction to enlarge its field and render its use even more valuable.

I am greatly indebted to my friend Dr. Charles A. Doremus, for his kind assistance in the demonstrations which accompanied this paper; especially for the skillful manner in which he projected the various spectra upon the screen.

[1] "Mountain-Ash Case." Reg. v. Robert Coe, Swansea Spring Assizes, 1866.

[2] Herapath, "On Use of Spectroscope and Micro-Spectroscope in Discovery of Blood-Stains," etc. *Chemical News*, English edition, xvii., No. 431, pp. 113 and 224.

EPIDEMICS OF THE CENTURY, AND THE LESSONS DERIVED FROM THEM.

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Civilization of modern times makes rapid progress, and a consideration of its yearly advancement and of the diseases incident thereto is a prolific subject of study.

In the review which is being taken by this Academy of the progress of medical science during the century of our national existence, the department of epidemics has been assigned to me for presentation on this occasion. The limits of a single discourse will admit only of a mere glance at this important topic, but the glance will be sufficient to show the industry and the success of our profession in elucidating the causes and in improving the management of those pestilential disorders which have appeared within our territory.

The period of review begins with the year 1776 and ends with the present year, 1876—a century peculiarly interesting to the American people, and pregnant with events which have moulded and which are still to mould the destinies of the world.

If we look across the ocean at the beginning of this period we find the indefatigable John Howard, of England, in the prime of life exploring the adyta of penal institutions and endeavoring to improve the morals and health of their inmates. At a later time, viz., in 1790, we find him fifteen hundred miles from home and, while pursuing his philanthropic duties,

dying in Russian Tartary. Princes, admirals, generals, magistrates and peasants united in paying him funeral honors—the land of the czars unheeding the fact that plebeian blood had nourished his royal heart. In the cathedral and valhalla of St. Paul in his native land can be read this description: He expired "a victim to the perilous and benevolent attempt to ascertain the cause of and find an efficient remedy for the plague. He trod an open but unfrequented path to immortality in the ardent but unintermitted exercise of Christian charity."

His biographer informs us that "His death fell on the mind of Europe like an ominous shadow: the melancholy wail of grief which arose on the Dnieper was echoed from the Thames and soon reëchoed from the Tagus and the Neva and the Dardanelles." No allusion is made to the effect which was produced on the shores of the mighty Hudson of the young American republic—the hum of a busy civilization was scarcely then audible on its thinly populated banks and the republic had scarcely assumed a recognized position in the brotherhood of nations. But our forefathers, imbued with advanced ideas of political and religious liberty, and differing in opinion from the nations of the Old World on these topics, were nevertheless ready to adopt the tenets of transatlantic philanthropy if originating either in the ages of a near or a remote antiquity or among contemporaneous peoples with whose political teachings they were at variance.

Nothing can better illustrate the American medical spirit of the period to which allusion is made than a glance at the brief but brilliant life of Dr. Elihu H. Smith. History demands a recognition of his useful career: I know not if a relative survives him.

Born in Litchfield, Conn., in 1771, he ripened into manhood at a time when Howard was prosecuting his benevolent work in Europe, and was but nineteen years of age at the time of Howard's decease. Settling as a young physician in New York, he was appointed physician to the New York Hospital, and became associated in 1798 with Dr. Samuel L.

Mitchell and Dr. Edward Miller in editing *The Medical Repository*, which was the first American medical periodical published in this country. The *first* paper in the *first* number of this journal was from the pen of Dr. Smith, and was entitled "The Plague of Athens." This classical essay can be read with profit at the present day.

The statesmen of that period were using their best exertions to strengthen the resources of the republic; the physicians with no less patriotism were endeavoring to shield the country from those pestilential besoms which in the Old World had so often inspired terror and induced desolation.

Dr. S., in concluding his paper, said: "If local causes originated a pestilence in Athens, local causes may generate a yellow fever in Philadelphia and New York. To these, then, be our attention more scrupulously directed, and let us be more solicitous in the inspection of our houses, yards, streets, and docks, than in that of cottons and woolens of vessels from the West Indies and ships from the Mediterranean."

Unfortunately for science, in the very year in which this paper was published, viz., 1798, Dr. S., while in his twenty-seventh year was smitten, in September, with yellow fever, then disastrously prevailing in this city, and closed his career.

The Medical Repository, in announcing his decease, says: "As a physician his loss is irreparable. He had explored at his early age an extent of medical learning for which the longest lives are seldom found sufficient; . . . he was ripe for the highest honors of his profession, his merits were every day becoming more conspicuous, and nothing but his premature fall deprived him of that extraordinary degree of public confidence which awaited a longer continuance of his life."

In the second volume of the periodical before mentioned is found a posthumous paper of Dr. S. "On the Pestilential Diseases which at Different Times appeared in the Athenian, Carthaginian, and Roman Armies in the Neighborhood of Syracuse."

Allusion has thus early been made to the labors of Dr.

S., whose career commenced with the century; and the efforts of others might have been cited with like propriety, in order to remind us that while the fires of genius are now burning brightly on our altars, they were not kindled by those who are within our personal recollection, but were ignited by those whose lives were identified with the early period of our national existence. The fact is also illustrated that then, as well as now, youthful enthusiasm helped to develop and to mould medical philosophy.

It would be unbecoming on an occasion like the present to give the details of the various epidemics which have visited our territory; it must suffice to group their general features and to evoke therefrom general deductions. Monuments to each pestilential visitation are thickly found in medical literature, and the explorer of to-day has merely to read their inscription to learn of their desolations, and to find not unfrequently the interment of various theories regarding the etiology and management of occult morbific influences.

In investigating our subject it will be found that several disorders which formerly were chiefly feared on account of their periodical general appearance have bequeathed to us venoms which have become naturalized in our midst, and which, though constantly and to a certain extent disastrously operative, are nevertheless prevented from exercising a very general pernicious influence by reason of the enforced sanitary regulations respecting them.

Our immediate territory is so far remote from the tropics that it can scarcely harbor and foster, for a long season at least, miasmata which are peculiar to the equatorial region of the earth. Nevertheless, we have a climatic season in which, by reason of the high temperature, if southern poisons are introduced, they may assume in our midst a pestilential character. Indeed, the ordinary maladies of our summer correspond in their nature with those which are observed in torrid situations, but are fortunately of less gravity.

Nations of the earth, in order to be regarded as mighty powers, must acquire a typical strength—such strength being

shown in the ability they possess for self-preservation. They must possess a force sufficiently vigorous to control their internal political and disturbing influences, and likewise a force capable of arresting the inroads of invading neighbors.

The inhabitants of the northern temperate zone—a zone recognized as the one in which man attains his highest mental and physical development, and the one in which our lot is cast—the inhabitants, I say, of this zone, being favored by natural causes, and by the advances of civilization, should feel it obligatory upon them to protect themselves not only from internal causes of disease, but also from the encroachments of inimical morbific influences from other zones, nations, and localities. Here intelligence and wealth, the resources of science, and the benevolent operations of religion, can best unite to develop manhood.

It can be clearly demonstrated that we of the present day, by reason of the advancement and diffusion of scientific knowledge, are better fitted than were our forefathers of a century ago to cope with various noxious agencies. Communities are not so readily panic-stricken as formerly at any threatened approach of pestilential diseases, being less superstitious and having more faith than formerly in artificial counteracting influences.

New sources of disease are developed by civilization—new measures of prophylaxis germinate simultaneously with each genesis. Many of the poisons with which we are familiar, and which were known to our ancestors, are not as yet to be extinguished, but are to continue to harass posterity. Posterity will encounter ills which have not afflicted us, and the medical literature of future centuries will relate the outcrops of novel disorders, and record the successful means by which they were held in abeyance. If the chemists are capable of producing new compounds, it can be inferred by analogy, and even inferred from a study of past ages, that in the mutations of society new forms of aërial contaminations and new zymotic affections will be developed by reason of the overcrowdings of population, by the influences of new occupations and of new

relations of life. The opening of new territories to civilization will be attended with its dangers. Indigenous germs of disease may exist in unexplored Africa and in other secluded parts of the globe, which are in time to be conveyed to marts of commerce, and thence to be still more widely diffused.

These prefatory and textual remarks lead us to a consideration of the epidemics which have chiefly excited interest.

The disorders which have thus appeared have been variously grouped. In the nosologies which have been accepted during the century, commencing with the one of Vogel in 1772, and subsequently in those of Sagar, Macbride, Cullen, Darwin, Crichton, Pinel, Parr, Young, Good, Hosack, and in the one adopted by our present Board of Health, as recommended by the International Statistical Congress, we find that the diseases under consideration have been classified in diverse manners, such variations being made to adapt the grouping to modified medical views respecting the etiology of the maladies.

The late Dr. Joseph M. Smith, in his "Report on the Medical Topography and Epidemics of the State of New York," submitted to the American Medical Association at its annual meeting in June, 1860, in writing on this subject, remarked: "Distributed according to their natural affinities or etiological relations, epidemics are reducible to three kinds, viz., *contagious*, *infectious*, and *meteoratious*. These terms are here employed in the sense they are used in the writer's work on the 'Elements of the Etiology and Philosophy of Epidemics.'"[1]

In such grouping, under the contagious epidemics were placed, small-pox, measles, scarlatina, and, as minor affections, varioloid and varicella; under the infectious, intermittent and remittent fevers, dysentery, yellow-fever, typhus, and compound fevers; under the meteoratious, influenza, typhoid pneumonia, diphtheria, erysipelas, and Asiatic cholera.

This classification, at the time, appeared as concise and as

[1] "Transactions of the American Medical Association," vol. xiii., 1860, p. 175.

accurate as it could possibly be made with the current scientific data. During the last decennary, the powers of the microscope as an instrument of etiological investigation have greatly increased, the spectroscope has developed new methods of inquiry, and experts have been in conference collecting observations and collating facts.

To say nothing at present of the careful study given to other epidemic disorders, what special attention has been given to cholera! An International Sanitary Conference, convened at the instance of the Austro-Hungarian Government, held at Vienna in 1874, devoted an entire month to a study of this malady. At the conference, Persia, Egypt, and every European state, were represented—an assemblage of delegates representing twenty-two nationalities. It is not a matter of surprise that, as a result of such advanced studies of epidemics, views concerning some of them have been modified.

Medical philosophy is now grasping after material or appreciable causes to explain morbid phenomena—theories, though admissible to investigation, must be sustained by observations which can, in part at least, be practically demonstrated by the chemists and microscopists.

I shall subsequently refer to the labor which has been expended upon the study of minute vegetable organisms and entozoa as causes of diseases. At present it must suffice to allude to the former of these.

There are so-called *filth-diseases*,* as appropriately denominated by Mr. John Simon. Excrementitious material from animal organisms, the refuse of households, factories, etc., either in rural or in city localities, may become causes of disease if not properly disposed of. Such excrement can largely be utilized to promote the welfare of the state in its agricultural interests, but, instead of its being thus employed or being rendered innocuous, it is constantly operative as a cause of disease and death.

These *débris* may become poisonous in a twofold manner: 1. They are liable to decomposition, and the inorganic gases thus developed, unless under circumstances permitting their

free diffusion, are injurious to those respiring them. Their effect, however, upon the animal economy is comparatively slight, and, the gases being merely inorganic, chemical compounds have no power of self-multiplication to induce further mischief beyond the persons of those who have inhaled them.

2. The *débris* alluded to are a fertile soil for the growth of noxious minute vegetable organisms. The refuse, if wisely distributed over glebes, rewards the laborer with turf teeming with cereals and rich in its production of edible vegetation, but, unwisely hoarded, this same material fosters the most luxuriant growth of innumerable microscopic vegetative growths, which bodies, being organic in their nature, possess the power of reproduction to an indefinite extent, even in other localities than those in which their development is specially favored. Some of these growths are believed to possess properties as conspicuously poisonous to mankind when gaining access to the system through the air, food, or water, as are the well-recognized benign influences upon the economy of the fruits of meadows and table-lands.

In the study of plants of the lower grade, Klein is asserted to have discovered the microphyte of enteric fever. In cholera, scarlet fever, diphtheria, vegetable growths have been observed, and, in various diseases, the excreta from the bowels, etc., are supposed to contain the germs of the prevalent morbid infections.

Vegetation, indeed, induces disease in vegetation. The Scripture says, "I smote you with blasting and mildew." "Blasting" and "mildew" have scourged the crops of the ancient Jews and Romans. The modern farmer employs the terms *smut*, *bunt*, *rust*, and *mildew*, to describe the more common diseases of vegetation, and, on a former occasion, as I have said before this Academy, these conditions are induced by vegetable parasites. Not a plant cultivated by man is free from such fungi. In the recent investigation of disease, as affecting the inferior animals and man, similar growths are believed to play an important part in their production.

But, while admitting such to be the case, there are doubt-

less other coöperative factors favoring the spread of epidemic disorders. There are atmospherical states which exert potent influences in promoting the diffusion of maladies. Those which are readily recognized, such as relate to heat, cold, moisture, dryness, electricity, calm, winds, etc., are observed to affect intensities of prevalence. As respects electricity, we appreciate the deadly result of the shaft of lightning—are there not obscure influences of this principle at times operative, concerning which we are unaware? The atmosphere is never in a state of stable electrical equilibrium, and it is unphilosophical to suppose that varying electrical conditions exert no influence, directly or indirectly, upon animate nature.

Respecting ozone, that mysterious, active, and condensed form of oxygen, how comparatively little is known as regards its general effects! Its presence in the air varies with the time of day and with the prevalence of certain winds, and is less appreciable in over-crowded localities than in uninhabited or sparsely-settled regions. We recognize it as the great oxidizing and purifying agent—an agent which is readily metamorphosed, for while speeding in its noble mission of reformation, and while in the act of purification, it is transmuted into the simpler form of oxygen.

Ozone, when undiluted, is irrespirable, and, when but moderately diluted, oxidizes the blood unnaturally and induces pulmonary irritation and systemic disturbance. It is readily inferred that, when this principle is present in a comparatively small quantity in the atmosphere, those respiring it are not thereby sufficiently vitalized, and that local mephitic agencies, bereft of a counteracting influence, acquire augmented morbific powers. When this same principle occurs excessively in the air, there is every reason to believe that other morbid phenomena are induced. More precise knowledge concerning this principle is needed, as well as of other principles of whose presence in the atmosphere we are already aware.

Is there not an "epidemic meteoration," or condition

which comprehends "insensible qualities of the general atmosphere, which produce or favor the prevalence of popular diseases?"[1] Investigations in this direction have not, as yet, been fully made. We have not, as yet, been furnished with instruments or means of inquiry sufficiently accurate to decipher occult influences of the atmosphere, and such an inquiry is essential, if we are to fully understand the wide-spread appearances of disease.

Water impregnated with certain organic and inorganic material is inimical to health; the atmosphere similarly contaminated is similarly noxious. The air is more complex in its chemical nature than water, and is, consequently, liable to greater vicissitudes in composition, and its principles, not all being chemically combined together, may be capable of behaving, under peculiar circumstances, in a mysterious way upon mankind. There is not that same stable constitution in air as there is in water *per se.*

While diffusion in the atmosphere prevents a universal and dangerous contamination from local pernicious causes, nevertheless, combined aërial and terrene conditions may render considerable regions of territory insalutary. A disease may be contagious through excreted germs, but for the disease to spread over extended areas the germ and atmosphere must be coadjutors; in other words, the germ must find a congenial medium in which either to sustain its life or to promote its growth, multiplication, and diffusion. It is, doubtless, true that the air at times is a mere vehicle of morbid germs, and such germs, when meeting congenial terrene or systemic conditions, there fructify, as the seeds of grass, etc., may be wafted by winds to develop in distant and fruitful soils.

Points of investigation specially needing elucidation at present are these: To what extent are meteoratious conditions alone responsible in the production of disease? To what extent are morbid germs culpable? What aërial conditions essentially favor germ diffusion? To what degree are inor-

[1] *See* "Elements of the Etiology and Philosophy of Epidemics," by Joseph Mather Smith, M. D., New York, 1824, p. 115.

ganic mephitic influences operative in the causation of disease?

Another point of interest relating to etiology is an inquiry into the nature of the development of a contagious element in the course of certain inflammatory diseases, and into the processes by which a multiplication of infectious germs takes place within the animal economy, irrespective of vegetative growth.

Puerperal fever occasionally seems to occur, even when the patient is under the most auspicious circumstances, and when there has been no appreciable inoculation with the disease, and from such a case contagion may arise and be diffused. It is common to attribute such a febrile genesis to a poison generated within the uterus, from decaying clots, portions of placenta, etc., which is absorbed into the system.

It has not been easy to explain the multiplication of the poison—the spread of the disorder. May it not be accounted for in this way? If innumerable *bioplasts* are eliminated from such a patient, is it not presumable that these minute organic particles are impregnated with the poison derived from the patient, and that they may become morbific germs? Indeed, the idea is now becoming prevalent that a number of diseases are contagious which have never been supposed to possess any such attribute. The method of their diffusion has not been clearly understood—infected *bioplasts* may be the morbid vehicles.

Vegetative growths may induce certain diseases, and specific poisons may thus be engendered in the animal economy. *Bioplasts* may be instrumental in spreading such septic conditions, and thus compound causes may coöperate in diffusing various maladies.

From what has been said it is evident that, concerning the specific natures of the poisons of a number of the epidemic disorders, we are almost entirely ignorant. We regret to have to acknowledge such ignorance, but have an ample cause of excuse. Some of these causes have been operative, at intervals at least, from time immemorial. But while this is a recognized fact, nevertheless, human genius has only been

qualified within a comparatively short time to engage in studies of such a nature. If we regard merely the eighteen centuries and three-quarters of a century of time according to our present computation of years, we can eliminate nearly eighteen centuries during which progress in such direction was at all practicable. We have scarcely to look back a decade of decades to find chemistry developing as a branch of scientific inquiry. We find hydrogen, oxygen, and nitrogen, and various other elements, shining out more brightly as the chemical discoveries of the century than the new lights of Uranus and Neptune in the contemporaneous centenary of astronomy. With the genesis of chemistry new philosophical attributes were unfolded to man, and with gradually-acquired gifts of analysis and synthesis, of eduction and production, he has clearly defined the operations of various occult atomic affinities, and is engaged in the definition of numerous mysterious phenomena of nature and of art. The microscope and spectroscope promise very material guidance in further researches, but as yet have afforded imperfect information, but the information already afforded has almost revolutionized medical philosophy relating to the subjects under consideration. While we deplore our ignorance concerning the precise natures of the poisons to which allusion has been made, we feel assured *lux lucet in tenebris*, and furthermore feel conscious, as the sequal will show, that we are already armed, through the resources of hygiene and chemistry, with means which have shorn pestilential disorders of half of their terrors.

We have found, in examining the etiological branch of our subject, that there are several paths of inquiry which have only been partially explored. With a limited knowledge in regard to vegetable parasites, and other organic germs, as causes of disease, and with a very limited acquaintance with the nature of atmospherical vicissitudes as inducing morbid conditions, with studies on these subjects at present being very intently prosecuted, it seems more difficult at this time to make a new nosological grouping of epidemics than it has

ever been before. We appear to be on the very threshold of developments which are probably to modify many of our preconceived ideas. Such being the case, I will not to-night attempt any new classification of diseases, but in treating of the disorders which have appeared as epidemics in our territory, as a matter of convenience, will merely speak of them in the order in which they have before been enumerated.

I will first consider the lessons which have been taught us by the visitations of small-pox, measles, scarlatina, and varicella, usually classified among the contagious disorders. As prefatory to such consideration it is desirable to say a few words in regard to *murrains* and *epizoöties*, which from time to time have prevailed, for between these diseases and epidemics there is often a close relationship, though not necessarily so, but it is particularly a noticeable occurrence between cow-pox and small-pox.

The prevalence of murrains has been recognized since a remote antiquity. In sacred history, Moses records, "Behold, the hand of the Lord is upon thy cattle which is in the field, upon the horses, upon the asses, upon the camels, upon the oxen, and upon the sheep: there shall be a very grievous murrain" (Exodus, ix. 3); and he further has described the disorder as "a boil breaking forth with blains upon man and upon beast" (Exodus, ix. 10). Profane history abounds in allusion to epizoöties. Homer in his "Iliad" relates:

> "On mules and dogs the infection first began,
> And last the vengeful arrows fixed in man."

In Œdipus is read:

> "For all those plagues which earth and air had brooded,
> First on inferior creatures tried their force,
> And last they seized on man."

It is unnecessary to multiply allusions to the numerous epizoöties which have prevailed in the world's history. The causes of these are in many cases as inexplicable as are those of some of the epidemics, but are being studied with interest and with most promising results. Those which have most re-

cently been observed in our territory have not appeared to have an intimate connection with human maladies, though in 1825 a disorder among horses, known as slavers, made a disastrous visitation, and preceded an epidemic of erysipelas and influenza throughout a part of this State.

It was a study of the relationship between vaccinia and variola, made about the commencement of this century, which led to the discovery of the great prophylactic of small-pox, viz., vaccination. Cow-pox made its appearance in England in 1745, and subsequently in 1770, when, as recorded by Aitken, "it appeared among the horned cattle with so much severity that his majesty George III., in his speech from the throne, at the opening of Parliament, on the 9th of January of that year, called upon the Houses of Parliament to take the subject into their serious consideration. The disease continued with more or less violence till 1780, and it was no doubt the expiring embers of this epizoöty which Dr. Jenner found in Gloucester, and made the basis of his investigation during that and subsequent years." In 1798, Jenner promulgated the result of his study in this direction. The medical profession of the civilized world has substantially adopted his views, and his name has almost been enshrined with that of Æsculapius.

It has not been an easy task to educate the popular mind into a belief in the prophylactic powers of vaccination, but three-quarters of a century have sufficed to accomplish the important work. With proper care, epidemics of small-pox can now be absolutely prevented, and when from neglect the disorder is encountered the patient need no longer be avoided by kindred and friends who have been properly protected. Certainly the century under consideration marks in this respect an epoch in medical history, and American physicians have been foremost in experimenting with and in advocating inoculation, and subsequently vaccination, as a means of preventing one of the most loathsome of human disorders.

Respecting the other contagious epidemics, viz., of scarlet fever, measles, and chicken-pox, the latter is ordinarily such

a mild affection as to require only passing notice. Of the others we unfortunately cannot speak so encouragingly as can be done of variola. Both prevail at times in an epidemic manner, their diffusion being favored by some unknown and peculiar condition of the atmosphere. This condition is not identical with that which propagates the meteoratious diseases, for there is not generally a contemporaneous prevalence of both classes of maladies. The poison of these maladies when once introduced, especially in the larger villages and in cities, appears hardly ever to be entirely eradicated.

Now, while we cannot boast of any special means of prophylaxis, we nevertheless find that a long series of years will elapse without an extended appearance of the diseases under consideration. This phenomenon can be accounted for in two ways: 1. Atmospheric influences do not always favor their diffusion. 2. When their diffusion is thus favored, increased vigor is employed in checking their spread. For example, the apartment of the isolated patient is thoroughly ventilated, and the poison, in only an attenuated form, can be wafted from the chamber or scattered by fomites. The removal of carpets and unnecessary furniture to prevent their infection, disinfection of the air, of the excreta, of the clothing, and destruction of some of the latter by fire, and, finally, fumigation of the apartment, are some of the means now resorted to with undoubted success to stay the progress of the disorder.

Among intelligent physicians these or kindred measures are voluntarily enforced to prevent the multiplication of foci of disease, and many municipalities enjoin such sanitary precautions. Prophylaxis in this direction has certainly made progress with the advance of time.

We now come to a consideration of another group of affections, viz., typhus and typhoid fevers, malarial affections, including dysentery and yellow fever, which have been classified as infectious epidemics. Typhus and typhoid fevers have an element in their character which allies them to a certain ex-

tent with the contagious disorders; the same remark is also applicable to dysentery, though perhaps in a more limited degree. A poison is disseminated from each patient suffering from these maladies, and those exposed while in attendance upon such patients are liable to become infected; but, in addition thereto, there appears to be a more or less diffused aërial condition favoring the diffusion of the diseases alluded to. Overcrowding of tenements, imperfect ventilation, defective trapping of waste-pipes, and faulty drainage, favor the local spread of the disorders, and the soil, becoming impregnated with morbific material, renders the water used as drink a vehicle of disease. From one or more of these causes have appeared at various times outbursts of the disorders under consideration in jails, ships, camps, villages, towns, and in scattered hamlets and rural situations. Armies, as is well known, have rendered the localities in which they were tarrying pestilential in the extreme, and on breaking up their quarters have by fomites carried with them devastation along the line of their march. But modern sanitary science has eradicated or modified many causes which in war, ocean-travel, penal and eleemosynary institutions, and among mankind generally, in segregated and gregarious life, have favored the development and spread of the febrile affections under consideration.

Respecting the malarial affections, our territory is so fortunately situated as not to develop those severe types characteristic of the same class of maladies encountered in the tropics. Intermittent and remittent fevers and dysentery are diseases, however, of sufficient gravity to intimidate the residents of regions in which they prevail. The prevalence of these disorders in distinctive paludal and littoral situations has left no doubt in regard to their etiology, though the precise nature of the miasm engendered in such places has never been absolutely defined.

On a former occasion I have shown that the Pontine Marshes spread infection in the territory of the Volschi, as they contaminate the same tract of country for its modern

inhabitants.[1] But the Italian morass, thirty miles in length and eight in breadth, has at times been partially converted into inoffensive and fertile plains by the thorough system of drainage enforced during the reigns of Augustus, Nerva, and Trajan, and during the pontificate of Pius VI. At other periods engineering resources were neglected; rich fields degenerated into impassable and noisome bogs, while the inhabitants of the environs lost their ruddiness and acquired an anæmic pallor.

Had the efforts toward reclamation been persistently made from the time of their inception by the consul Cethegus, a baneful spot could have been permanently obliterated; stagnant waters could have been collected into running streams, quagmire converted into loam, and the low vegetation of a vast fen metamorphosed into genial fruitfulness. The earth itself would have become as magnificent a monument of the philosophy and grandeur of ancient Roman civilization as have been the classical literature and massive architecture of the historical period alluded to.

In modern times there have been imitations of the ancient methods of drainage, in numerous localities, and with the most satisfactory results, but, while we find it practicable to improve the health of certain localities by the means indicated, it seems to us at present almost a fruitless task to attempt to reclaim a large part of an entire continent by means of such sanitary engineering skill. To escape injury from irremediable regional morbific influences, the inhabitants must invoke the resources of private hygiene and establish physical vigor capable of withstanding baneful surroundings.

In regard to yellow fever, we find its poison engendered and most intensified under the equatorial sun—in a zone where vegetation is monarch, and where aboriginal man, enervated and to a great degree secluded by reason of climatic causes, as regards his highest attributes, is no more typical of manhood than vegetation is typical as found on mountain

[1] Anniversary Discourse, New York Academy of Medicine, Gouverneur M. Smith, 1870.

eminence or in polar latitude. The poison, however, diffuses itself over semi-tropical regions, and is conveyed to the temperate zone, and here, during the summer solstice, especially if favored by a hot and humid atmosphere and insalutary local conditions, it can for a season operate disastrously. But the resources of science step in to arrest its operations. Experience has taught that wisely-administered quarantine regulations, conjoined with local sanitary precautions, can either blockade its entrance into northern ports, or at least deprive it of resources favoring its epidemic diffusion. In corroboration of this fact, we have simply to study the frequent threatened invasions of the port of New York, and of various cities on our southern seaboard, and specially the sanitary history of New Orleans during our recent war.

Respecting the latter subject, our distinguished Fellow, Dr. Elisha Harris, in a paper read before this Academy, January, 1865, on "Hygienic Experience in New Orleans during the War," etc., appropriately said: "Such immunity from her accustomed scourging of yellow fever had not been enjoyed by New Orleans the last half century. Even her wisest hygienists had been generally discredited, and often derided, when they publicly taught, as Fenner, Barton, Simonds, and Bennett Dowler had most faithfully, that the active and localizing causes of yellow fever, and the high death-rate in that city, were preventable. There was a truthfulness worthy of the medical profession in the words of Dr. Barton, who, as president of the New Orleans Sanitary Commission, sitting in grave and scientific consultation upon the terrible visitations of yellow fever, unhesitatingly declared the causes of that pestilence and the city's excessive insalubrity '*entirely susceptible of cure.*' But how few persons appreciated the truth of Dr. Barton's words of prophecy, when he said that 'upon the broad foundation of *sanitary measures* we can erect a monument of public health, and that if a beacon-light be erected on its top, and kept alive by proper attention, this city will be second to none in this first of earthly blessings!'"[1]

[1] "Bulletin New York Academy of Medicine," No. 30, September, 1865.

I come now to consider a group of affections varying greatly in their character, as regards both the severity and the nature of their symptoms. I allude to influenza, a comparatively mild affection; pneumonia typhodes, a grave disorder; diphtheria, a malady of similar gravity; cholera, a distemper universally dreaded; and erysipelas, likewise serious in its character. These maladies, when appearing in an epidemic manner, are in some instances apparently caused, and in other instances perhaps merely diffused, by atmospherical conditions, the precise nature of which we are for the most part ignorant. In three of them, symptoms of irritation and inflammation of the respiratory tracts are particularly noticeable; in one of them lesions of the *prima via* are as peculiarly marked, while in another the dermic tissue is specially involved.

These disorders have been before grouped as meteoratious affections. Diphtheria and cholera are now recognized as contagious diseases; but their prevalence is at times so peculiar and general that it has been difficult to ascribe their spread to contagion alone. It seems proper, in our present state of knowledge, to admit that the germs of these maladies, as derived from patients, meet with aërial and terrene conditions particularly favorable to their diffusion, and consequently many more people are smitten by them than those coming in contact with the sick.

The poisons of the other affections have seldom, in this country, offered opportunities for study. Their sudden and wide-spread appearances seem to teach that atmospherical conditions are important factors in their production.

Epidemic influenza and typhoid pneumonia have only occasionally been observed here. Diphtheria, while it has prevailed in our territory as early as 1751, and at a few subsequent periods, had attracted little attention until within a few years. Of late, especially in our large cities, its poison has become acclimated, and like the variolous virus is constantly operative, but not always in an epidemic manner. The disorder possesses a contagious element. While little can be

done to correct any general condition of the atmosphere which favors the diffusion of diphtheria, yet the local measures of purification now in vogue are potent in preventing the extension of the disorder by contagion, and by thus destroying the local foci of disease we prevent also its diffusion by meteoratious influences.

Repeated opportunities for studying epidemic cholera both on shipboard and on land have been afforded during the century; but science has failed to define specific choleraic germs. The study of the subject, however, has not been nugatory. The conditions favoring the development and spread of this disorder are allied in character to those favoring the outbreak and diffusion of several other forms of pestilence. The germs of the disorder are supposed to be microscopic vegetative growths; such, indeed, is the present prevalent opinion. The perennial habitat of its peculiar poison is India; and the world, to a greater or less extent, is perennially inoculated with cholera from this great focus of the disease.

Routes of travel are its favored highways. The traveler may convey by luggage, etc., from an infected region the germs of a poison which, under favoring circumstances, may smite him with death in a distant land. A case thus developed is capable of spreading the disease to those about him through the material vomited, ejected from the bowels, or eliminated by the kidneys, unless such oral, anal, and renal excreta are disinfected either by a strong acid or by similar potent means of destruction. When such measures are not resorted to, and large numbers of cases are suffering from the disease, the general atmosphere may become so contaminated as to carry the poison for some distance; but the distance to which it can be meteoratiously spread is uncertain. I cannot enlarge on this topic, but would remark that, if our sanitary guardians keep informed in reference to the foreign prevalence of the malady, steps can be taken here to prevent either its introduction or its general prevalence. A recent report, published under the auspices of the United States Congress, very truthfully says: "Nothing is more clearly proved by the his-

tory of cholera than that epidemics of this dreaded disease can be controlled by *vigorous hygienic measures. The true remedy against cholera is preventive medicine.*"[1]

Regarding epidemic erysipelas, it may be said that few opportunities have been offered for studying its peculiarities. The disorder is kindred in its nature to the one originating in our hospitals, but differs from it in appearing over large sections of territory.

Another disorder to which no allusion has been made occasionally prevails in an epidemic manner, and is peculiarly distressing on account of the class of persons affected thereby. Reference is made to *puerperal fever.* While it may not be possible to prevent sporadic cases of the malady, nevertheless, its ordinary prevalence in lying-in asylums is attributable to gross and culpable carelessness, and, with our present knowledge of the etiology of the disease, its conveyance by accoucheurs, attendants, etc., is an iniquity scarcely less abhorrent than that of inducing criminal abortion. The efficient means now employed to prevent the outbreak and spread of the distemper in maternity hospitals, and to prevent the portability of its poison from these institutions and from cases in private practice, and of congeneric poisons to women about to be confined, if properly carried out, can almost eradicate the affection. Such prophylaxis is a result of the intelligent study of the subject within a comparatively few years.

No more interesting field of study regarding epidemics is to be found than that relating to armies when engaged in active service. During the world's history, the javelin and lance, the sling and falchion, the tomahawk and bayonet, shot and shell, have killed their hundreds of thousands; but, while contending arms have sacrificed innumerable hecatombs of lives, during these same strifes disease has proved a more disastrous foe of valiant battalions than inimical weapons of war. The countless belligerent forces which have from time to time been mustered, containing the youth, the pride, the flower

[1] "Cholera Epidemic of 1873 in the United States," Washington, 1875, p. 19.

of the earth, and whether or not engaged in holy or unholy warfare, have time and time again been desolated by disease. Indeed, an almost decimating decimation has occasionally withered the power of what has seemed an invincible force.

Ancient history has embalmed the memories of surgeons of former days. We linger when reading of the care bestowed upon the wounded Machæon, we mourn when learning of his decease, we rejoice at the divine honors paid to his memory, and hallow the temple erected in his honor at Massenia. But the medical officers of those days were chiefly lauded for their surgical abilities—their modern brethren must possess skill of a wider latitude.

The causes which have induced disease and death among soldiers in their active campaigns until a comparatively recent period, if not overlooked, have not, at least, received that prophylactic attention which humanity demanded. As lately as during the Crimean war—a war in which civilized nations were contending, in which the prowess, the strategy, the engineering skill, and the mighty armaments of strife of the nineteenth century, were on trial and being scanned by the world—a waste of human life was permitted from preventable diseases which has cast a stigma on nations, the bravery of whose soldiers has elicited the plaudits of continents.

Our own republic was plunged soon after in the horrors of civil war—a war which called into the service, including reënlistments of veteran volunteers, an army of loyal citizens numbering 2,753,723. The lessons learned from the Crimean war had been studied on this side of the Atlantic. "We were left to no vague conjecture as to the causes which produced the fearful mortality among allied troops before Sevastopol—a mortality which, as has been truly said, has never been equaled since the hosts of Sennacherib fell in a single night."

The proclamation of the President of the United States, calling for troops, was issued on the 15th of April, 1861, and with its echo was heard the tap of the drum, simultaneously firing the patriotism and philanthropy of the North. The

recruit was scarcely under marching-orders when means were being devised to protect him from disease.

On the 23d of April, 1861, but eight days after the President's manifesto, an association of physicians was formed in this city known as the "New York Medical Association for the Supply of Lint, Bandages, etc., to the U. S. Army." The object of the society was not alone to furnish means for the treatment of wounded, but to provide as far as possible, to the regiments gathering at the front, hospital clothing, etc., for rapid equipments included bare necessities; and recruits would almost necessarily suffer from disease in their sudden transition from civil to military life.

The society had an existence of three months, and numbered fifty-five physicians, many of them Fellows of this Academy. During this time eighteen hundred dollars in money was collected and judiciously expended; vaccine virus was distributed to various regiments and the hospital at Fortress Monroe; numerous articles gathered from the benevolent of an estimated value of $11,548.46 were carefully dispersed to thirty-six regiments, six hospitals, to the Naval Department, to the Medical Purveyor U. S. Army, and articles of diet placed in a proper channel to reach those requiring them.

While this and numerous other initial local steps were being taken to aid in maintaining the physical integrity of the army, wise measures were being devised to concentrate into a single organization the benevolence of loyal States. As the result of such deliberation the U. S. Sanitary Commission was organized on the 12th of June, 1861. While the Government was sufficiently animated in its efforts to preserve the national Union, it scarcely appreciated the importance of sanitary regulations to maintain the health of troops, and it needed the stimulus of an association fresh from the people to arouse it to a full sense of duty in this regard. The project of infusing civil aid in military administration met with disapprobation in certain official quarters.

It would be impossible on an occasion like the present to describe the work performed by this association—an associa-

tion fostered at its incipiency by several of the distinguished Fellows of this Academy, and aided in various ways by a number of us, both here and "at the front," by voluntary services. Can a few sentences or paragraphs epitomize its operations?

Under the auspices of the Sanitary Commission, military, medical, and surgical monographs were prepared relating to the prevention of disease, and to the hygienic as well as medical management of the disorders and injuries incident to war. Salutary advice was afforded regarding the proper sites for camps and for the sites and proper modes of construction of hospitals. A corps of special inspectors of the general hospitals of the army was appointed, composed of gentlemen of known repute. No less attention was called to the dietary of troops, and through the agency of the association scurvy was prevented in some sections of the country, and its ravages arrested in others. Special care was taken for the moving of sick and wounded by means of thoroughly-equipped ambulances, hospital cars, and transports.

Armies stationed in unhealthy localities were made objects of special surveillance. At New Orleans the commanding general and Dr. McCormick, medical director of the department, were alert in guarding the soldiers from disease. "The fear of the outbreak of yellow fever during the summer months, and the danger to which a Northern army would be exposed by its prevalence, acted as a constant stimulus to the most careful measures of prevention[1] . . . . Early in July, 1862, the whole number of sick in that department, in regimental and general hospitals, was only four hundred and seventy-two out of a force of about twenty thousand men (nineteen regiments of infantry and seven batteries), less than two and a half per cent. . . . This favorable state of health among the troops in the Department of the Gulf was maintained during the whole war. In November, 1863, the experienced inspector of the commission, Dr. Crane, writes: 'I have never seen so little disease among troops in the field. But little over four per cent. of the present force is on the sick-list.' . . . It

[1] "United States Sanitary Commission Memoirs," Philadelphia, 1866.

is certainly very remarkable that a far higher health-rate was maintained during the war among the troops on the coast of Carolina and the delta of the Mississippi than in the mountainous regions of Tennessee and Virginia."

Never before in the world's history had such powerful forces been gathered and been protected by sanitary care—the voluntary gifts to provide for such protection amounting to nearly five million dollars. It was the enlightened care on the part of the people and of the Government of their troops which contributed in no small degree to the maintainance of the integrity of a nation whose centennial birth-year we now celebrate. The lessons derived from the war are not to be forgotten, but will prove of value to coming generations. In future conflicts of the earth Hygeia will assert her supremacy beside that of Mars, and the horrors of war be gradually and gradually diminished, until the time shall come when

> "No more shall nation against nation rise,
> Nor ardent warriors meet with hateful eyes,
> Nor fields with gleaming steel be covered o'er,
> The brazen trumpets kindle rage no more;
> But useless lances into scythes shall bend,
> And the broad falchion in a ploughshare end."

The sanitary experiences of the late war have taught lessons which had never been so thoroughly impressed, and which are as important respecting civil as military life. They have shown that insalutary places can be made comparatively salutary, and have almost shown, to use the words of another, that "vanquished Nature yields its empire to man, who creates a climate for himself;" they have illustrated the fact that men may be congregated in vast numbers, and yet not be mercilessly ravaged by disease; they have proved that a proper dietetic regimen will arrest the progress of and prevent certain disorders; they have demonstrated that pure fresh air, which is so lavishly and beneficiently distributed by Nature, is a more powerful factor in the relief of sick and wounded than medicinal herbs and minerals, which are also benefi-

ciently provided, but are offered more sparsely, and as mere adjuvants to the breath of life.

The results of these and of other teachings derived from the same experience are already apparent, and have influenced public opinion. As an illustration of this, I need only refer to the prevailing views regarding hospital construction. While the disadvantages of ordinary infirmary buildings had long since been observed, never had communities been brought to fully realize their imperfections until, on a large scale, the advantages were shown of treating patients either in tents or in inexpensive, simple, commodious, and thoroughly-ventilated pavilions. Such structures are as well adapted to the treatment of civilians as of soldiers; and, while for civil purposes they should be built in a more durable manner than those employed during periods of war, nevertheless, they should not be very permanent in their nature, but be destroyed after a comparatively few years of an intermittent occupancy. Mr. James W. Beekman, in writing on this subject, has well said: "The architectural necessities are light, air, speedy removal of refuse, and great facility in cleansing. Architectural display in a hospital is a crime. 'Do not build for a long futurity.'"

While such axioms are becoming household words, they are dispersing knowledge of a wider significance than their literal interpretation would convey. If the homes of the sick must be models in their sanitary arrangements, an antithesis of like importance is believed to be true, and as a result increased attention is being paid to the proper construction of all kinds of human habitations, and more attention is being paid to the selection of their sites, etc.

In this brief and desultory review which I have given of my subject, it may be said that little has been advanced of a positive nature in regard to the specific poisons which occasion the epidemics and zymotic disorders which have been considered. I have refrained from giving in detail the various prevailing theories regarding them, and one reason for such omission may be found in the fact that the most accom-

plished experts shrink as yet from defining with certainty the precise nature of these subtile deleterious agents.

The medical world is now engaged, as I have before said, in a special study of minute vegetable organisms, to say nothing at present of the lower forms of animal existence and of the influence exerted by them in various diseases. They are found in certain processes of inflammation, in tissues, in secretions and excretions, and in the atmosphere. We are seeking intently for the biography of parasites, for information regarding their germs, their growth, their longevity, and their death. We are endeavoring to learn at what periods of their existence they may be offensive, and whether or not it is the germ or the parasite which is capable of being conveyed or of remaining in a passive condition awaiting favorable circumstances for increased growths. Ordinary vegetation affords edible and poisonous growths; microscopic vegetation is not destitute of potent properties.

Kindred to this subject of vegetable parasites is that branch of science which has been developed during the century, viz., helminthology or entozoölogy. Between thirty and forty human entozoa have been carefully described. Many of these in their adult life are of such magnitude as to have early attracted notice. Moses doubtless alluded to dracunculi or Guinea-worms when describing the "fiery serpents which afflicted the children of Israel during their stay in the neighborhood of the Red Sea" (Cobbold), and Agatharchides doubtless alluded to the same helminth in his clever account of the "little snakes" affecting the limbs and muscles of the inhabitants of the same seaboard.

While several animal parasites were of sufficient size to have early attracted notice, nothing was definitely known, until comparatively recently, concerning their origin, their larvæ, their growth, their migrations, and again several entozoa were so diminutive in size as to have altogether escaped observation until discovered by microscopic assistance.

A number of diseases prevail among inferior animals attributable to entozoa, and among mankind from similar causes.

Several of the former appear as epizoöties and of the latter as epidemics. Of the epizoöties, the more noticeable are the *gapes*, a disorder prevalent among poultry and among a number of birds depending upon the presence of scerostomata in the trachea, a disease first described by Dr. A. Wiesenthal, of Baltimore, in 1799; the *rot-disease*, especially destructive to sheep, and caused by the presence of the fasciola hepatica, or liver-fluke; the disorder among swine attributable to trichinæ; the malady among swine, dogs, wolves, etc., due to tæniæ. Of the epidemics due to entozoa, the most interesting for present consideration are those due to tæniæ and trichinæ.

How greatly has our knowledge been increased within a few years concerning the cestoidea or tapeworm group of helminths, due in great measure to the experimental study of the subject by Von Siebold and Küchenmeister! How carefully have cystic worms and larvæ been observed, their migrations noted, and the diseâses induced by them in their various stages of development been described!

What interest has been awakened in regard to nematoidea or round worms, especially as relates to the trichina spiralis. The history of the life and peculiarities of this parasite has been as carefully depicted as if it was one of our familiar domesticated animals. Zoölogists have carefully described to us the fauna of continental areas and of epochs, and are now furnishing us with volumes relating to entozoa.

As a result of such labors, agricultural interests have been promoted by preventing diseases among important inferior animals, and mankind has been further benefited by escaping a number of loathsome diseases. Fortunately our country has not been very seriously invaded by this class of maladies. Further labors in the direction indicated must be prosecuted; unrecognized entozoa are doubtless inducing mischief, and the habitats of those which are known are not fully appreciated.

In the civilization of our century, Europe, Asia, Africa, and America, are freely interchanging commercial commodities. While gems and bullion, cereals and fruit, fabrics and

manufactures, are being widely distributed for the comfort of mankind, let science prevent the diffusion of entozoic fauna by preventing human contamination with them in the respective continental habitats of these obnoxious helminths.

Zoölogical investigations relating to parasites are in advance of botanical researches concerning minute vegetable organisms, but while this is so, we are gathering important information regarding vegetative germs as effecting cholera, diphtheria, typhoid fever, and other disorders. Such studies are being prosecuted with the same zealous spirit as that which actuated Linnæus, Wolff, and Goethe, in other departments of botany, and are to be crowned with a similar success. As yet we stand at the threshold of the branch of science under special consideration, and are merely pioneers. In this respect, we may almost be said to be contemporaneous with Zoroaster, Aristotle, and Plato, in their study of the general subject of botany.

This being the case, the question arises: Have the benefits of sanitary science been overestimated? The ultimate nature of light has not been absolutely defined, but its salutary effects are clearly intelligible. The precise nature of electricity is not fully understood, but its nature and properties are sufficiently appreciated to be utilized in an almost miraculous manner. So various occult morbid influences are known to be operative by the prevalence of peculiar phenomena. The causes favoring such prevalence are often discernible, and if not directly discernible the causes favoring their prolonged operations can be clearly observed and appropriate prophylactic measures can be resorted to. While neither municipal, State, nor Federal legislation can enact and enforce laws so governing the private life of a citizen as to promote his healthy growth, nevertheless jurisdiction can impose laws which restrain an individual from jeopardizing the lives of his neighbors by his sanitary negligence, and can regulate matters relating to public hygiene.

The enlightened nations of the earth are now paying special attention to hygienic legislation and to the promulgation of

sanitary instruction. Sovereignties and municipalities alike are vigilant in guarding the general physical welfare of the people. Intelligent boards of health are now established throughout our land in the larger cities, and the villages are instituting similar provisions of safety. This salutary state of affairs, which has comparatively recently been inaugurated on so comprehensive a scale, has been due to the persistent assiduity of our profession in pressing upon communities the important results to be derived from such organizations. While the boards of health alluded to may at times fall into unskilled management, such transfer can be but temporary, as the people are already convinced that scientific skill must wield the helm in such departments, and will never consent to revert to that condition of sanitary apathy which but a few years ago was so apparent to medical men.

But we must not be content in urging alone the establishment of local boards of health. State boards and national boards must be organized, having a recognized official authority. Between the local boards and those of the state and nation there must be authoritative communication, and the national boards of the various countries of the earth must unite in holding official correspondence relating to matters of the public health. Some such provision of safety will assuredly become established.

Already we recognize the advantages to the nation from the daily reports of the War Department relating to the weather. The numerous meteorological observations made at distant points, and diurnally, at fixed hours, telegraphed to Washington, are concentrated under scientific supervision into terse dispatches, and daily published throughout the land, giving the areas of storm and dryness, of heat and cold, and foretelling the approach of tempest, of calm, and sunshine—thus aiding commercial, agricultural, and other interests. A somewhat similarly arranged sanitary department must take its place among the national bureaus.

A few more thoughts must be offered on a subject to which slight allusion has only been made. When epidemic causes

of disease are prevalent, to say nothing of the personal means suited to induce vigor, to say nothing of ventilation, of drainage, sewerage, etc., of domiciles and localities, as aids in escaping injury, chemistry affords efficient agents to neutralize pernicious local conditions which are contaminating the atmosphere. These agents have chiefly been presented by modern science, and are the outgrowth of the century.

Human ingenuity, however, has long been exercised in this direction. While we may now deride many ancient customs, some of which were of superstitious origin, and many arts of the alchemists, we must bear in mind with respect many primitive means of disinfection and preservation. Jewish priests exercised hygienic supervision of households and used appropriate means in purifying the clothing and dwellings of those afflicted with the leprosy. Herodotus and others have recorded the manner of embalming the dead. Egyptians early understood the processes of drying to prevent decomposition. Ulysses burnt sulphur to destroy the infection arising from dead bodies, and Roman shepherds burnt sulphur and herbs when sacrificing to Pales, the goddess of their sheepfolds.

It is unnecessary to multiply examples of this nature which illustrate dim insights into a science into which we are peering, and even still quite cloudily. Nevertheless, progress has been made, and we possess a group of disinfectants which may be classified as follows: 1. Those which destroy or prevent the multiplication of the lower forms of organized bodies; 2. Those which have the property of absorbing deleterious principles and thus rendering them inert; 3. Those having the power to decompose various noxious agents into less poisonous or innocent principles. This classification as a matter of convenience is sufficiently accurate for our present purpose and partially exhibits the precision aimed at by modern science. Besides these we recognize the potent agencies of heat and cold.

"In such indexes,
There are seen
The baby figures of the giant mass
Of things to come at large."—SHAKESPEARE.

The chemical agents which we can now utilize as disinfectants are more numerous and, in many respects, are vastly more potent than any hitherto offered, and animate us to further exploration and further achievements as the land-growths wafted to the ship of Columbus stimulated to a prolonged and successful search for our Western Continent.

Entering on a new century under most favorable auspices, it can safely be predicted that before its ten decades have been spent, measures of prophylaxis and of therapeutics will be established of far more importance than any of those which during the previous history of the world have been developed by scientific ingenuity.

DEGENERATIONS OF THE PLACENTA AS A CAUSE OF THE DEATH OF THE CHILD.

By CHARLES A. LEALE, M. D.

Read May 18, 1876.

The degenerations of the placenta, commencing during the early months of pregnancy, affecting the life of the fœtus, offer a vast field for medical scientific investigation, and the knowledge of their causes, prevention, and cure, will enable the physician, in many instances, to guard against a frequent cause of abortion in the anæmic, and in those suffering from constitutional diseases, which, as many authors have asserted, is one of the most frequent causes of producing a change in the structure of a tissue absolutely necessary for the development and existence of the fœtus; and when we are called to witness the sorrow of women whose intensest desire is to have offspring, but who, after suffering all the miseries incident to a long pregnancy, at the end of nine months are to be again disappointed, we are surely called upon, not only to tender our sympathies, but to investigate the cause, and if possible, avert the recurrence of such a catastrophe. Having notes of the instances where this condition has occurred during the past ten years, I will present for consideration a few clinical facts which may fortify us in the future, and lead us to consider the best measures to adopt to prevent what is one of the most disagreeable duties of a physician, viz., to deliver a woman of a dead child.

By fatty degeneration of the placenta we mean that gradual transformation into fat, and in the end complete loss of usefulness and final destruction of the tissues, found in a healthy

placenta, which may be often recognized by the naked eye, either as yellow solid fat, yellow liquid fat, or granular fatty masses, found occupying the former positions of the fœtal tufts and villi of the spongy portion of the placenta. In the tissues of the umbilical cord, and in the membranes surrounding the fœtus, we find the fatty degeneration of a granular form, as numbers of small elevations feeling like shot, completely destroying that transparency of healthy membranes, and causing the amnion to become dirty-looking, yellowish and opaque. As will hereafter be explained, it will be seen that we have the yellow softening, the result of the occlusion or retardation of the passage of blood through the nutrient vessels as we have found in the syphilitic form.

The cheesy degeneration, or fatty granular metamorphosis of Virchow, as in one from a patient expiring in the last stage of phthisis pulmonalis. The atrophic fatty degeneration also found in a placenta from a mother having pulmonary phthisis. The waxy or lardaceous, from a very gross scrofulous mother, in which no free fatty infiltration could be found.

Paget states that "in most instances the fatty degeneration affects, first and chiefly, the contents of cells or tubules, or the proper substance of the membrane. And when it thus happens, the nuclei almost always waste, and either shrivel or disappear after gradually fading in their outlines. This may be commonly seen in the fatty degeneration of the renal and hepatic cells, and of the muscular fibres. The whole history of fatty degenerations concurs to prove that they are the result of defect, not of disease of the nutritive process; and that they may therefore be classed with the atrophy which we recognize in merely diminished quality of formation." Therefore we can easily imagine why a woman, who has either conceived with a diseased sperm, or is herself the subject of syphilis, phthisis pulmonalis, scrofula, etc., from simply that defect of nutrition and malassimilation, can have a degeneration of a fatty character start in a placenta, and thereby be the direct means of preventing her from giving birth to living children at the full term of gestation.

Illustrative Cases.

Fatty Degeneration of the Placenta originating in Sperm from Syphilitic Father, the Mother never having had any of the usual Manifestations of the Disease.—At a meeting of the Pathological Society, April 22, 1874,[1] I presented a placenta weighing only six ounces, discharged from a lady confined at full term. Throughout the substance of the organ were three lobes of fat, each about the size of a small hen-egg, which could easily be seen through the thick membranes of the fœtal portion of the placenta as semitranslucent masses, while a very delicate transparent membrane was all that covered them on viewing the organ from its maternal surface. They were situated at equal distances from each other, and midway between the insertion of the cord and periphery of the placenta; lines drawn from their distal extremities would have represented a right-angled triangle. On removing the delicate membrane on the maternal surface they were found to be cavities filled with liquid fat, having their base and sides lined with soft yellow fatty tissue, showing complete destruction of at least one-third of the entire placental tufts. On the surface of the amnion over its entire extent were seen a large number of granular fatty spots, the size of small peas, which destroyed the usual transparency of healthy membrane giving to it a dirty yellowish and thickened appearance.

The infant was a miserable, puny, and wrinkled boy, weighing about five pounds, having barely sufficients, trength to cry.

The mother, up to the time of marriage, had been a very healthy, rosy-cheeked, robust lady, but soon after conception she changed in appearance, became pale, fretful, and extremely haggard-looking, losing all energy and becoming so sensitive to slight variations of temperature as to have frequent attacks of bronchial irritation and leading her relatives to suppose her to have chronic pulmonary trouble. On examination

[1] *Medical Record*, p. 302, 1874.

her heart and lungs were found to be in an excellent condition. She had been treated by several physicians for a variety of troubles, yet from none did she derive any benefit. She had never had, to her knowledge, any sore on genital organs, nor any cutaneous eruption, nor any of the usual symptoms of syphilis. When I first saw the father a short time after the birth of the child he was asked how long since he had had syphilis, which was answered by a positive denial, but inadvertently said that he had been troubled with a sore throat for several months, which on examination proved to be syphilitic. On being informed of this fact, he acknowledged that he not only had had chancre, but also had been covered with the secondary eruption ; that he had been told he was cured, but had always since been troubled with a tender sore throat. On examining the glans penis, distinct syphilitic mucous patches were then visible.

Simple Fatty Degeneration from Placentitis, the Result of a Contusion.—Mrs. K., at the eighth month of her pregnancy, while returning from Brooklyn, was forcibly thrown forward in a collision, causing her to strike against the abdomen near the umbilicus. Considerable fever followed, but she went her full term, and I delivered her of a paralyzed child, with club feet, and having a large hæmatoma over parietal bone. The child died in a few hours after its birth ; the mother had a good recovery. The placenta showed evidences of placentitis, was adherent to the uterus, and evidently had been the seat of inflammation after the accident.

Fibrinous Deposits in the Placenta ; Hydrocephalic and Rickety Child, the Father having Morbus Coxarius, and the Mother Cavities in both Lungs.—E. F. R., aged twenty-two years, in consequence of phthisis pulmonalis, was advised not to marry. She had a severe cough of over three years' duration, frequent night-sweats, and latterly three quite profuse hæmorrhages from the lungs. Her intended was deformed by hip-disease, there being about three inches shortening, and a false joint.

They did marry, and her health immediately began to im-

prove after conception had occurred. There was no nausea, but her cough continued, with profuse expectorations of thick muco-purulent matter, fully four ounces in some days.

She did not believe herself to be pregnant, and supposed the stopping of her courses due to extending disease of the lungs. She came to me for advice, and was then told to walk and ride in the open air, eat hearty nutritious diet, and do all to increase her strength, and to return at the end of the following month. Signs of pregnancy had now commenced to appear, by discolorations around the nipples, increased size of breasts, and abdominal distention. She still had a hard cough, and cavities in the lungs could be easily detected. Night-sweats occasionally occurred.

April 17, 1867, after an easy labor she gave birth to a very feeble child, of eight months intra-uterine life. The mother had a tedious convalescence, but her cough diminished in severity, and after the first month she rapidly gained strength and flesh. The child was fed on goat's milk, and throve very well. He is now over nine years old, and very precocious, with a head twenty-one and a half inches in circumference. He promises to be much stronger than either parent.

The mother has since had two children, each the subject of chronic nasal catarrh. She easily gets cold and has a protracted cough follow, but is to-day in far better health than on the day of her marriage.

The placenta with each child contained large fibrinous deposits.

Tuberculous Degeneration of the Placenta; Child born at six months in consequence of and during a Violent Paroxysm of Coughing; Death of Mother thirty-seven hours after Delivery; Death of Child twenty-four hours after its Birth.—September 29, 1868, I saw, for the first time, Mrs. P., aged twenty-eight years, the mother of one living child, and on examination found cavities in both lungs, and a large remaining portion the seat of chronic solidification with extensive pleuritic adhesions; each thoracic cavity contained about a pint of liquid. She stated that during the past year

she had had almost constant fever, frequent night-sweats, and had rapidly lost strength.

She did not suppose herself to be pregnant until after feeling the movements of the child for some time, which were always feeble; the cessation of her menses, she was told, was in consequence of disease, far advanced, in the lungs.

When I arrived, she was so much exhausted that we all expected death to occur at any moment. I had not been in the house many minutes, and before examining the uterus, the child was born during a very violent paroxysm of coughing; the placenta soon followed. Very little hæmorrhage occurred, and by means of pressure on fundus uteri it was kept firmly contracted. Brandy revived her a little, the cough was much less severe. She became cyanotic, bathed in perspiration, and died in thirty-seven hours, from insufficient lung-tissue to support life.

Syphilitic Fatty Degeneration of the Placenta.—On June 25, 1868, I visited a lady who aborted at her sixth month, a few moments after I arrived at her bedside, she at the time telling me that during the preceding six years she had several times aborted without any known cause, as she strongly desired to have children. The fœtus was not so well developed as a child at that term, although the bony skeleton was as mature, yet there was a great deficiency of flesh, which hung loosely about the bones, the skin being wrinkled, the child having evidently died from absence of proper nutrition.

The placenta was found to have undergone such extensive fatty degeneration as to prevent the free circulation of blood through its placental tufts: it was carefully kept until the following day, when Dr. S. T. Hubbard saw her. He then stated to me that at her previous abortion he had also found as the cause fatty degeneration of the placenta. I then endeavored to find a cause, but after careful inquiry and examination found none. Three years after, the father came to my office for copper-colored stains on skin of forehead, and acknowledged that, over fifteen years prior, he had had a very severe attack of syphilis.

Apoplexy and Calcareous Degeneration of the Placenta.—October 30, 1875, I delivered Mrs. S. of a boy weighing eleven and a quarter pounds. The father and mother were each twenty-six years old. The mother was perfectly healthy, had been married six years, this being her fourth child. The father, during his wife's last pregnancy, had been very hysterical, and on one occasion, after considerable scolding, had fallen to the floor, as his wife supposed in an attack of apoplexy. On my arrival I found him grinding his teeth and having muscular contortions, which they were trying to overcome by forcibly restraining him on his back on the floor. The mother passed through all this excitement and did her part to control her husband. While I was in the house a telegram was received stating that their youngest child had been in convulsions for the past twelve hours, and for the parents to come immediately a distance of over 200 miles. The mother then was four months advanced in pregnancy. After this all went on very well until the second day before delivery, which occurred at the end of the fortieth week of gestation, when she suddenly became very faint, while reaching to put clothing away on high shelves. Her labor was normal. The placenta was large and had a number of partially-organized clots in its substance. There were also a number of calcareous masses about the size of pigeon-eggs, which I supposed to have been the result of changes occurring at the period of excitement five months before delivery.

The following case illustrates that cancer may not affect the placenta.

Extensive Cancerous Disease of the Os Uteri; Apparent Cure; Subsequent Conception and Delivery of Healthy Child at Full Term; Reappearance of the Disease; Death from Exhausting Hemorrhages seven months after Delivery; No Disease of the Placenta.—During the autumn of 1868, Mrs. E. P. C., aged twenty seven years, was treated by me for cancerous erosions of the os uteri which had caused frequent and profuse hæmorrhages; after three months' treatment, I produced an apparent cure by the application of fuming

nitric acid to all indurated and ulcerating parts, leaving the stump covered by a pale, healthy-looking, but almost bloodless mucous membrane. After this she became regular and did not have any ichorous, watery discharge between the menstrual periods, and on viewing all the parts to be seen she appeared to be cured. In a short time she became pregnant, had no trouble, and at the end of nine months, on December 6, 1869, I delivered her of a healthy girl; the disease very soon reappeared, was checked as much as possible until the last of June, when the ulcerations had caused a vesico-vaginal fistula. June 22, 1870, she was examined in consultation by Dr. Thomas Addis Emmet. The disease extended rapidly and death soon followed from exhaustion.

In the *Medical Times and Gazette*, September 5, 1874, a report of the Obstetrical Society of London is given, where at the meeting of July 1, 1874, Dr. J. C. Hays exhibited a fatty placenta taken from a patient who furnished a very instructive history. She was thirty-seven years of age; married sixteen years. There were no grounds for any suspicion of syphilis. She bore three living and perfectly healthy children. During her fourth pregnancy she received a severe fright, and shortly afterward was delivered prematurely of an eight-months still-born fœtus. Her health then began to fail, and in succession followed twelve still-born children, nearly all of which had reached at least seven months' maturity. During the last four or five pregnancies she had suffered from serious and frequent hæmorrhages, dating from the third month of gestation, and occurring sometimes every fortnight till labor supervened; they were quite sudden and without any recognizable cause, and came in gushes not unfrequently during the night, unattended by pains. When first seen, February 7, 1874, she was four months advanced in pregnancy, and had had two serious losses of blood. She was very anæmic and extremely debilitated. Rest was enjoined, and fifteen minims of the liq. ferri perchloridi prescribed thrice daily. Her color and strength

rapidly came back, and there was no return of the hæmorrhage. Fœtal movements were distinctly felt until within six days of labor, which happened at full term, and the fœtus, though small, bore all the appearances of maturity, and seemed to have been dead about a week. Since the birth of her last living child gestation had never continued so long. The placenta was small, weighing only twelve and a half ounces; it had a pale-yellowish appearance, contained little blood, and nodules of fat, characteristic to the naked eye, were studded over the maternal surface, especially along its margin.

The fatty change was most advanced toward its surface, a large portion of the decidua being merely an aggregation of oil-globules. The adjoining fœtal villi, with the investing chorion, were seriously affected, but those close upon the fœtal surface were simply granular in appearance.

Dr. Hays, after referring to the investigations of Dr. Barnes, published in the *Medico-Chirurgical Transactions*, stated that he considered the disease in this case to have originated in the maternal portion of the placenta, this view being borne out by the weakly condition of the patient and the microscopical examination.

No doubt the previous premature births and the hæmorrhages had been due to the same condition of the placenta. He could not regard the disease as originating in placentitis; this was contrary to all analogy. It was a degenerative change, occurring under the same conditions which induced fatty heart, fatty kidneys, and fatty arteries in the brain. Should the patient become again pregnant, the propriety of inducing artificially premature labor would be considered.

Dr. Barnes thought that the case proved that the disease may originate during the life of the child. This notion had been controverted. It was said to take place after the death of the child. But it was hardly possible that all this mischief could have occurred in the short time after the death of the child; it was very much more likely to have preceded this event, and, in fact, caused the premature destruction of the child. As regards the change commencing in the maternal or

fœtal portion of the placenta, it affected mainly the villi of the chorion. He likened it to fatty degenerations of other organs, and had not stated that the change arose from placentitis. The case was full of interest and had important pathological bearings.

Had the vice-president of this academy and one of their own honorary members been present at that meeting and known the results of a case almost analogous in every respect terminating during his absence, he could have given conclusive evidence that this change in the placenta does occur during the life of the child, and that on the preceding June, in New York, a result had been obtained in proof that his theory was founded on fact.

I will now give an illustrative case showing that extensive fatty degeneration of the placenta occurs during the life of the fœtus *in utero*, and that the induction, artificially, of premature delivery can accomplish the birth of a living child, which now at the age of three years is in excellent health and has a fine physique, the mother in this instance having become very anæmic in consequence of six rapidly succeeding pregnancies, where the death of the fœtus, or that of the child immediately after birth, was caused by degeneration arising in the placenta.

Mrs. B., aged thirty-seven years, at the time of marriage, considered herself to be in excellent health, not conceiving until the termination of the ninth month after marriage, when she almost immediately began to lose flesh and have constant nausea, which caused such debility that her husband spent the following season with her traveling in Europe. Mrs. B. continued to grow worse, and at end of her seventh month gave birth to a very feeble child, which lived about five minutes. Eleven months after she conceived the second time, and almost immediately her stomach rejected the food, and uncontrollable nausea and prostration followed; at the end of seven months and ten days, during a violent attack of vomiting, she was delivered of a dead, decomposing fœtus; profuse hæmorrhage followed for several weeks, leaving her very anæmic. In

thirteen months she conceived the third time, nausea and vomiting reappearing, and a disinclination to take nourishment. This fœtus also she lost at six and a half months, her attending physician then stating that the child had been dead over two weeks *in utero.*

Shortly after this, while still very anæmic, she became pregnant for the fourth time, increasing very rapidly in size after the sixth month. At seven months the pressure on the diaphragm, from the increased size of uterus, in consequence of an abnormal accumulation of amniotic fluid, became so painful, that her only rest was while in the erect posture, and her only sleep was while she rested her arms, shoulders, and head on her piano (a table about forty inches high). This condition continued for two weeks, when, at the end of seven and a half months, acute lancinating pains in the diaphragm became so severe that general prostration ensued, almost proving fatal by syncope. Soon after rallying, uterine contractions commenced, and a very feeble, wrinkled child was born, only having sufficient strength to live for two hours. Her exhausted condition and frequent disappointments now caused her to be very melancholy; she refused sympathy, thinking that she could never accomplish her chief desire, viz., to have a child live. A change of residence to new scenes and associations enabled her to become both mentally and physically better, but yet quite weak, when she conceived for the fifth time, and placed herself under the care of Professor Fordyce Barker, who gave chlorate of potash and iron, at the same time paying particular attention to general hygiene. She felt motion up to the end of the eighth month, when she complained of abdominal pains, for the relief of which morphia was given. She went about one week longer, when Dr. Barker delivered her of a child, it having been dead about four days. Dr. Barker now had an opportunity to examine the placenta, which he found to be far advanced in fatty degeneration, and determined, should she again become pregnant, to produce premature delivery whenever he found the fœtal heart's action becoming feeble, or signs of prostration

in the mother alarming, waiting, if possible, until after the end of the eighth month.

In May, 1873, just prior to Dr. Barker's departure for Europe, he requested me to take charge of her, at the same time giving me a history of her previous and present pregnancies, and stating his conclusions.

She had then been married about twelve years, this being her sixth pregnancy; she was very anæmic, the mucous membranes of mouth and conjunctiva being almost pearly-white; yet she was more cheerful, and endeavored to do all that was possible to regain strength.

On June 1st I visited her, and found very little change in her condition, and, in accordance with suggestions, examined all urine passed during the previous twenty-four hours; it measured twenty-eight ounces, was of a light amber color, specific gravity 1020, and did not contain a trace of albumen or sugar. Dr. Barker's directions were continued, nothing of importance occurring until the morning of June 24th, when I was hastily called to see her, and found that, in consequence of having eaten a large quantity of stewed green rhubarb on the previous night, she had acute indigestion, which in a few hours had caused severe vomiting and purging, and had lasted all night. On my arrival in the morning she was in a very much exhausted condition; the vomiting had ceased, but the purging and cramps were severe, accompanied by violent attacks of tenesmus. She was requested to lie down in bed, and while the hand was on abdomen a slight movement of the fœtus was felt.

On listening, the fœtal heart could be heard and its pulsations counted; the os uteri was rigid and not in the slightest degree dilated. With hand on her abdomen, and frequent listenings to strength of fœtal pulsations, careful observation was continued for two hours, there being considerable difficulty in controlling her excitement, as well as the nervousness of her husband and relatives.

By this time I noticed that the strength of the fœtal heart-sounds was gradually failing, and the mother was becoming

hysterical. I then told them that it was time to follow Dr. Barker's advice, and immediately began to induce premature delivery, all willingly assenting. I commenced by gently pressing the forefinger against the rigid os uteri, and counter-pressure with opposite hand made over the fundus. The pressure was so gentle that it took nearly an hour to dilate the os sufficiently to allow the end of finger to enter, when the uterine contractions began with moderate severity, and occurred at regular intervals. This procedure was continued gently for five hours, when severe uterine pains began to such a degree that the father, in his excitement, urged me to save the mother, and not mind the child. At the end of one hour's severe pain she gave birth to a small, wrinkled, and nearly dead child, not having sufficient strength to cry, only occasionally emitting a feeble whine, characteristic of the weak and premature child. It was immediately submerged, except face, in a bath of water at 100° Fahr., to keep it as warm as while *in utero*, and, after a very superficial washing, wrapped in warm blankets (not dressed), and the body surrounded by bottles of hot water, to supply that heat which its feeble condition rendered it incapable of generating. A few drops of water were given to the child, and it was left with the nurse to care for the mother, who became very much excited, apprehensive of losing her child. The placenta, by gentle traction and pressure over fundus uteri, was expelled entire, and, on examination, was found to have undergone extensive fatty degeneration. *Post-partum* pains of mother were relieved by morphia in quarter-grain doses.

The child, when it was about twenty-four hours old, became cyanotic, cold, and gasped for breath, and was now too weak to emit the peculiar feeble whine of premature and dying children; fresh, soft flannels were warmed, in which it was wrapped, and spirits of camphor, on a small piece, directly applied over surface of abdomen. The artificial heat, by means of bottles filled with hot water, was continuously applied, and the little one fed on its mother's milk, which had been drawn by the nurse. For six days and nights this was

continued, requiring the alternate attention of three experienced nurses, and almost the entire time of the attending physician. On the sixth day the child had hepatitis, became decidedly jaundiced, had great tenderness over the liver, with marked enlargement and distinct induration, probably arising from umbilical phlebitis, although the navel appeared normal. The warmed applications of camphor on flannel were constantly kept on its abdomen, and, to relieve constipation, a piece of flaked manna, the size of a filbert, dissolved in water, was given, producing the desired effect in a very gentle manner. On the eighth and tenth days even the sclerotic membrane of each eye became of a canary-yellow color. The jaundice was very persistent, lasting more than six weeks, and all this time the child continued very feeble. When the child was three weeks old, a small red papule, about the size of a large pea, appeared on the inner side of the left knee; it was three lines in diameter and one in elevation, of a dirty-red color, and had uneven edges. In six days partial desquamation, but no suppuration, occurred, leaving a decidedly copper-colored, saucer-shaped depression, which soon became of a darker copper color.

Nothing at first was done for it, and at each visit the mother would inquire if I could not give her something to remove "that copper-colored spot," which seemed to annoy her very much. The nurse was directed to wet it thrice daily with sweet oil, so that its progress might be observed, and inspection of the entire skin of child was made. Not a single other papule appeared on the skin. The mucous membrane at anus, and the surrounding integument, at the age of four weeks, assumed a red, coppery hue, which resisted the usual applications for simple excoriations. The palms of hands and soles of feet were more red than usual, and wrinkled. The child continued very feeble, but was able to nurse.

The parents stated that they did not remember ever having had any eruption, but I decided to treat the child by the inunction of the unguentum hydrargyri fort., a piece the size of a bean to be rubbed in groin and under the arms, also

over abdomen, under the flannel band, twice daily, and around the excoriations at the nates. The inunctions had been applied only a week before the irritated condition of the anus improved, and the copper-colored depression, which had also been anointed, commenced to fade, gradually doing so until the end of two weeks, when all cutaneous eruptions had disappeared; the inunction, however, was continued in all nearly three weeks without producing any salivation, the child continually improving. The child at the age of ten months had never had any return of the trouble, and continued to grow rapidly. It had no teeth, fed well, was cheerful, but rapidly developed at the frontal portion of cerebrum. The bones of the skull were semi-translucent, and the blue veins in temporal region distinctly visible; the bones of the legs and arms appeared normal, and intellectually the child was unusually bright. At twenty-six months old I again saw her; she had been several voyages on the ocean, had remained in the country during the warmest part of the summer, had passed the winter months in the South; she had sixteen teeth, was very active, and physically and intellectually appeared as well as most children at the same age.

The mother, previous to marriage, had enjoyed good health, but her frequent abortions and disappointments had caused her to become very anæmic and melancholy; she became so desponding that, at one time, fears were entertained that she might lose her reason. She spent the following season in Europe, and under Dr. Barker's treatment and guidance improved so much that, as the previous history shows, she became a happy mother.

The father, aged fifty-two years, denied ever having had syphilis; he had not been in good health for three years, and two years previous to the birth of this child had placed himself under my care. He stated that he had been almost constantly employed for twenty years, and for the past five years noticed failing health; he then had constant headache, and a temperature of 100½° Fahr.; had had chronic bronchitis for several years, and was nervously prostrated; had occipital and tem-

poral pains whenever he took any violent exercise. On examination of his urine, no albumen, sugar, or casts were found; he had no valvular disease of heart, but that organ was exceedingly irritable, and, following the slightest excitement, it would violently palpitate, and the temporal arteries would become very much distended; as he told me, felt like bursting. My diagnosis in note-book at that time stated: "Commencing atheroma of cerebral arteries and fatty degeneration of muscular tissue of heart."

Conclusions.—Degenerations of the placenta are a more frequent and unrecognized cause of the death of the fœtus than is generally supposed; these degenerations may commence early in pregnancy, and slowly progress until over one-half of the entire fœtal tufts have been rendered useless before causing the death of the child.

Cause of Degenerations.—Uterine or ovarian abnormalities; insufficient vitality of ovule; disease of ovule; insufficient vital force of the sperm; disease of the sperm; emotional causes from shock producing either paralysis, or partial or incomplete detachment of the placenta.

Varieties of Degenerations of the Placenta observed during the past ten years.—Tuberculous degeneration; calcareous degeneration; degeneration with extensive fibrinous deposits; anæmic degeneration, with atrophy; simple fatty from primary placentitis; fatty degeneration from apoplexy of placenta; fatty degeneration from scrofula; fatty degeneration from syphilis; waxy degenerations from leucocythemic mother.

The tuberculous placenta, to the naked eye, very much resembles the same condition found in the lungs, it being a solid, heterogeneous mass, not capable of being normally inflated, and on transverse section showing the yellow fibrinous deposits, surrounded by tissues altered by inflammation, with cavities filled with pus and degenerated cheesy matter.

Apoplexy of the placenta I believe to be caused by either violent rupture of the blood-vessels of the organ, or in consequence of some degeneration, where, as the result of overreaching, tension was exerted on the fragile, calcareous,

inelastic vessels, easily breaking them, and thereby permitting extravasation of blood into the surrounding tissue.

In placentas from very anæmic, emaciated mothers we find different conditions. If the exhaustion of the mother has been very rapid for a few weeks prior to child-birth, we have portions of the fœtal villi representing a recent change into fat. In others, where apparently this fatty change has occurred at some more remote time, we find a shrinking, with little remaining liquid oil-globules or fatty tissue, while in other portions of the placenta the lobes have entirely disappeared, and only a dense fibrinous substance representing how the organ has become contracted.

Fibrinous deposits in the placenta have only been observed in those from mothers who are themselves very anæmic from some protracted blood disease, and whose blood is deficient in red corpuscles. The deposits have been observed in placentas thrown off as early as the seventh month, and, from the intimate connection of the deposits with the underlying villi, appear to have been the direct cause of preventing the proper nutrition, and therefore effecting the death of the child.

Fatty degeneration of the placenta caused by syphilis manifests itself when the mother, as is usual, has an abundance of adipose cellular tissue, by several of the lobes being transformed into fat, which is apparent to the naked eye, being seen through the membranes as yellow masses, and on opening are found to be cavities filled with liquid fat, which can be poured out, and resembles the yellow olive-oil, the empty cavities being surrounded by a bright yellow placental substance filled with microscopic oil-globules.

Primary placentitis may be caused directly by violent contusions. After inflammation of the placenta has occurred, firm adhesions may follow, rendering it difficult to detach the after-birth from the womb. This is most readily done by introducing the entire hand into the uterus, and, with the little-finger on the ulna border edge, pass it beneath the placenta, and remove the entire mass with the hand from the uterus, when it generally remains contracted. This procedure I

have several times resorted to, and not in a single instance have known any unpleasant condition follow.

In waxy degeneration, as a rule, we have generally an unusually large placenta, which, when the uterine contractions have been protracted and severe, may be expelled, almost entirely freed from blood, and to the naked eye looks like a homogeneous mass, which, on being cooled, becomes hard, and can be easily cut into slices. Microscopically examined, under a high power, a profusion of very minute glistening oil-globules, in unbroken cellular tissue, can be seen.

The children are generally like the parents, gross, fat, and phlegmatic, having a superabundance of the white blood-corpuscles.

The blood from the uterus is of a lighter yellow buffy color than that from healthy, robust parents.

In regard to the treatment of either one or both parents. when syphilis is supposed to exist, the mercurial vapor-bath, will probably give a better success than by any other methods

In phthisis pulmonalis, a gentle cathartic, if necessary to produce one movement from bowels each day, a generous diet, good hygiene, and the inunction of entire skin of body three times a week after a general bath.

In scrofula the preparations of iodine, and, if admissible, iron, both in very small doses, viz. : tr. iodini co., mj. ter in die.

In profound anæmia, where all treatment has either failed or is not admissible, where a degeneration of the placenta has been known to cause the death of the child, and the mother becomes again pregnant, and the same condition is feared, the artificial induction of premature delivery, at about the thirty-sixth week, gives the most favorable opportunity for the mother to have a child capable of living, as recommended by Dr. Hays and Dr. Barnes, at the London Obstetrical Society, July 1, 1874; and, on the preceding month (June), successfully accomplished, I believe for the first time, by Dr. Fordyce Barker, of New York.

INCISION AND DISCISSION OF THE CERVIX UTERI.

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Read June 1, 1876.

If "meddlesome midwifery is bad," meddlesome surgery is not less so; and that form of it which attacks the uterus is the worst of all, since it not only injures the patient herself, but also compromises, and perhaps destroys, her prospects of offspring. Trachelotomy,[1] or cutting the cervix uteri, has been of late so indiscriminately, and often so unnecessarily performed, as to suggest this general remark. And, though perhaps somewhat less common, and with some operators less severe than five years ago, a reference to the most authoritative treatises on gynæcology shows that it has not yet reached its legitimate limitation even among gynæcologists; while many general practitioners operate as frequently and as blindly as ever, it being so facile a procedure that nobody hesitates to attempt it.

But trachelotomy in some form must continue to be recognized in the treatment of many cases of dysmenorrhœa and of sterility when depending on stenosis of the cervical canal; and the least hazardous, if equally curative, should be preferred. I propose to consider its usual methods, and their uses, abuses, and actual value; and also to explain a new method, which, I maintain, includes all that is valuable in them and still more, without their objectionable characteristics.

The two authoritative methods of trachelotomy hitherto

[1] From τράχηλος, neck, cervix, and τέμνω, to cut or incise.

practised, with some modifications hereinafter to be specified, are:

1. By Simpson's metrotome, or some modification of it; deep incision of the cervical canal.

2. By the scissors—discission of the cervix, or Sims's operation.

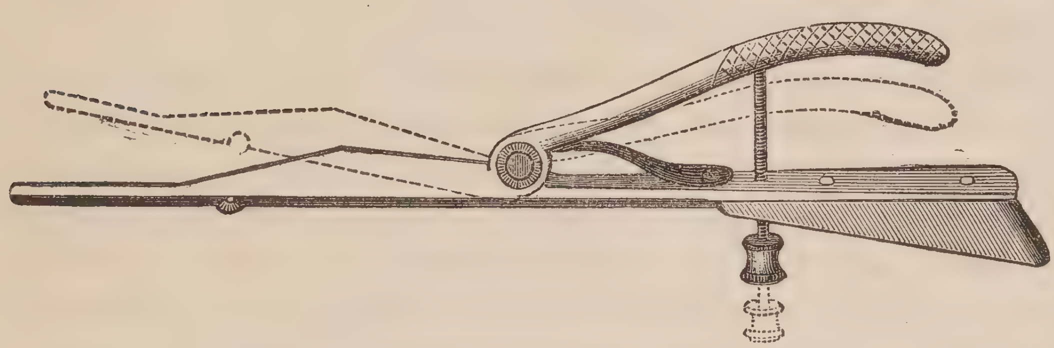
To these I add a third method, first suggested by myself, and which I will designate as "superficial trachelotomy."

Each of these methods will be separately considered.

I. Deep Incision of the Cervix Uteri—*Simpson's Operation.*

Prof. Simpson, of Edinburgh, maintaining that the stenosis producing dysmenorrhœa and sterility exists usually at the internal, and not at the external os uteri, in 1844 devised his metrotome for overcoming the constriction. It is shown by Fig. 1, and is too well known to require any special description. I improved it, I think, some fifteen years since,

Fig. 1.



Simpson's Metrotome (one-third size).

by lengthening the sheath to the extent of three-quarters of an inch beyond the end of the blade, so that the full strength of the fingers can be brought to bear upon the blade without displacing the sheath, in case the uterine tissue requires to be divided without the application of traction at the same time. Subsequently, Dr. Greenhalgh, of London, proposed a two-bladed instrument, it being merely a double, as Dr. Simpson's was a single *bistourie cachée.* This divided the cervix symmetrically, or very nearly so, as Dr. Simpson's instrument did *not*,

except by chance; and, generally, more extensively than the latter was intended to do. I might also mention several other metrotomes devised in this and in other countries, were they of any special importance to my present purpose. These are almost all double-bladed, and act on the same principle as Dr. Greenhalgh's instrument, which soon became generally preferred to Simpson's. I therefore speak of its action especially in what remains to be said under this head; there being, in fact, no practical difference between its effects and those of Simpson's metrotome when freely used, as by its originator himself.

Advocated by so able a defender, Simpson's operation soon became quite common in Great Britain and this country, though it was not accepted on the Continent till 1860. Its range of application also became extended. For while, rationally, it was at first invoked only in cases of stenosis of the cervical canal, including of course the two ora, it ere long became common enough in cases in which no obstruction at all had existed, and as a mere matter, as it were, of fashion. During a sojourn abroad in 1866, I witnessed a number of operations which I could place only in this category; and several of the same kind have I since seen at home. It seemed to be generally assumed that the uterus is an organ quite indifferent to cutting and hacking; and that the deep incision would at any rate do no harm. Indeed, it was known that Dr. Simpson had often performed his operation at his consulting-rooms, and afterward sent his patients home in a cab; and I cannot learn, up to the present time, that he ever reported an adverse case in his experience. It is, however, stated on unquestionable authority, that some of those patients died in consequence of the operation, and others narrowly escaped death; and it is proper that I here definitely specify its immediate and its remote effects. Of course it is to be recollected that I am speaking of deep incision of the cervix, as performed by means of Greenhalgh's metrotome, or of Simpson's, in a bold hand like that of its originator. And I have to consider:

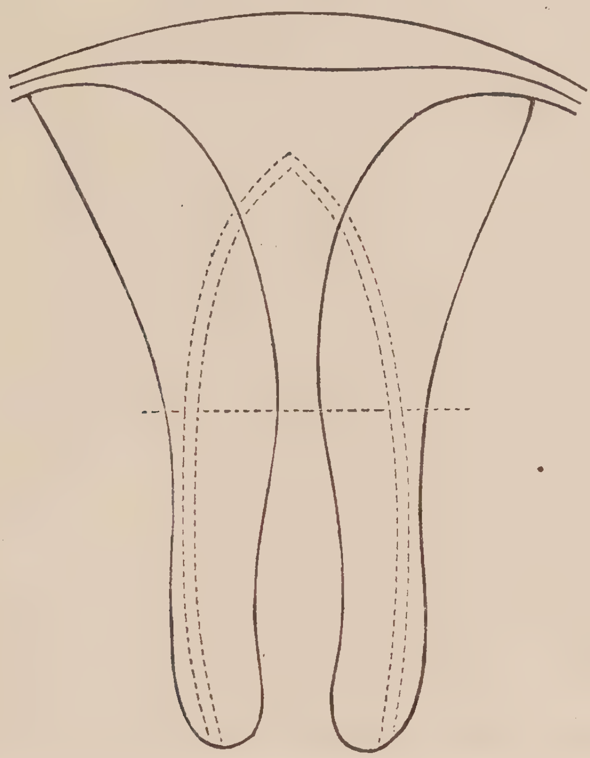
1. The change in the shape, size, and relations of the whole uterine cavity by the deep incision.

2. Its immediate dangers.

3. Its remote effects.

1. *The change in the shape, size, and relations* of the cervical canal, produced by the deep incision, are shown by Fig.

FIG. 2.



LARGE AND SMALL CUT BY GREENHALGH'S METROTOME; from Hewitt.

2, where the normal size and shape of the canal are seen, while the dotted lines outside of it show the "smaller and the larger incision," as Dr. Hewitt calls them, made by Dr. Greenhalgh's instrument. It is seen that the internal os after these incisions is somewhat more than three times its normal width, while the rest of the canal is increased in nearly as great proportion; and that the whole cavity of the uterus, cervix and corpus together, no longer retains its normal form as shown by Fig. 10, but resembles an erect, wide-necked, flattened flask without a bottom (Fig. 13). We shall see whether such a cavity can be depended upon as a receiver or as a retainer further on.

This operation ignores the importance of the normal relations of these cavities, even more than would one, were such a procedure possible, which should permanently dilate the urethra to the size of the small intestine. I fully assent to Dr. Sims's criticism, that this operation cuts altogether too extensively; an objection which will, however, be seen to apply as truly to his own.

2. *The immediate dangers* of so deep a division of the cervix are—a profuse, and sometimes even a fatal, hæmorrhage, pelvic cellulitis, and septic peritonitis, which is almost always fatal. It is known that these results occurred to some of the patients operated on by Dr. Simpson at his consulting-rooms. There is also a risk of cutting through the cervix into the peritoneal cavity.

Such effects can, however, surprise no one who is aware of the extent of the lesion produced. Indeed, it is surprising, rather, that they are not even more frequent than they are actually found to be. Referring again to Fig. 2, it will be seen that the walls of the cervix are cut more than half through on both sides, to a considerable extent, by the lesser incision with the two-bladed metrotome; while, by the greater, should there be a slight inclination of the uterus or of the instrument to either side, or a slight thinning of the uterine walls, an opening would be made into the peritoneal cavity. This has actually occurred. The entire cervix may also be split completely through, as in a case mentioned by Dr. Sims (p. 171).

A free hæmorrhage, at least, is inevitable in such circumstances. And even the lesser incision may divide one or more arteries on the level of the internal os, as shown by Figs. 3 and 4, where it is seen that in the first preparation there is an artery within one-eighth of an inch, and in the other, three arteries within one line, of that opening. The lesser incision cuts to the depth of at least three-sixteenths of an inch, and the greater to more than one-quarter. If sufficient for all practical purposes, it is therefore of the greatest importance to restrict the depth of the incision of the internal os within the

limits of the arterial distribution; and which I shall show may be done.

Fig. 3.

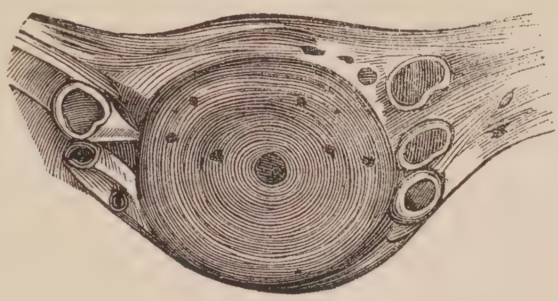


Fig. 4.

Arteries on Level of Internal Os; Barnes, p. 207.

The danger of pelvic cellulitis and septic peritonitis are referable to the fact that the medullary portion (*Savage*) of the cervix is laid open; and that thus perfect facility is afforded for the absorption of septic matters. This also may be avoided.

Hence Dr. Barnes[1] regards "incision of the internal os as being attended by great danger. First, there is profuse, even 'furious' bleeding. Next, from the gaping of the divided veins, and the injury of the tissues in which they run, there is liability to pelvic inflammation and septicæmia. These are

[1] Dr. Barnes's objection to Dr. Greenhalgh's instrument, because it acts automatically, is not, I think, well taken. If an instrument acts automatically, but always within certain perfectly well-known and safe limits, thus enabling the skillful surgeon to succeed, and securing the bungling operator against an accident, it is not to be rejected merely because automatic. But if an automatic instrument acts at random, or always beyond certain limits of safety, causing both the scientific and the ignorant operator equally to do harm, it is certainly very objectionable. This I hold to be the real objection to Greenhalgh's instrument.

no theoretical dangers. Many cases, some fatal, are well known" (p. 206). He says that "an incision even one-fourth of an inch deep will be very liable to divide some of the vessels;" and, therefore, though he still sometimes uses Simpson's metrotome, he cuts only from below the internal os to the external.

I am obliged to speak thus generally of the dangers of Simpson's operation, since no statistics have ever been published, as should have been done, especially by those who have operated most frequently.

From the facts I have given it may be inferred that no amount of experience in this operation will prove a safeguard against its dangers. And the following case illustrates this, as well as the unpardonable carelessness which great familiarity with an operation sometimes engenders:

A lady, twenty-eight years of age, who had been married eight years without ever having been pregnant, applied to me, several years ago, to remove the cause of sterility. She had no dysmenorrhœa, no uterine displacement, no stenosis of the cervical canal, nor, indeed, any uterine symptoms at all, excepting a slight leucorrhœa from congestion of the endometrium. This having been cured, she soon after left, with her husband, on a short summer-trip to Great Britain and the Continent, I having advised her, if any uterine symptoms returned, to get the advice, while in Edinburgh, of the then most distinguished gynæcologist there. No uterine symptoms did return; but, while in that city for two or three days, she decided not to lose such an opportunity to obtain his advice respecting the sterility, and sent for him. After a rapid examination, he remarked that a very slight operation was required; and at once introduced his metrotome and incised the cervical canal, and left the room within about three minutes afterward. The husband left about five minutes after the physician, and did not return for an hour, when he found his wife had fainted from loss of blood, which had saturated the bed and escaped upon the floor. Rushing to the doctor's residence, he found the latter had gone five miles out of the city,

to the wedding of one of his assistants; and, going next for another assistant, he also was found to have gone thither. The nearest physician was then called in, who found there was no time to be lost, and arrested the bleeding by continued pressure, till the operator could be brought back to the city. Returning, he remained with her the next twelve hours. Her life was barely saved, and, after passing the summer in that apartment, she had only recovered sufficiently to be able to travel; and, the time for the journey to the continent being exhausted, she returned directly home. She required more than a year for the recovery of her strength and color; and now, at the end of seven years, still remains childless. The surgeon had performed the operation, probably, more times than any one else in Europe, and must have previously found this operation to be a treacherous procedure.

3. Having found the immediate effects of this operation to be thus undesirable and even dangerous, I now proceed to consider *its more remote results*, both curative and otherwise.

As a remedy for dysmenorrhœa and sterility, when depending on stenosis of the cervical canal, Simpson's operation usually succeeds, temporarily at least, with the former; and as generally fails with the latter. As the contents of a bottle without a bottom can have no difficulty in escaping from it, so the menstrual fluid should have none in leaving such a uterine cavity as that shown in Fig. 13. And, if pain still attends menstruation, it is of course due to some other cause than stenosis. The incision, however, not seldom gradually closes up, in spite of the surgeon's intentions, and the relief proves to be but temporary; and sometimes the cicatrix, continuing to contract, finally reproduces the dysmenorrhœa in a severer form than existed at first.

On the other hand, it can scarcely be expected that such an enlarged open cervical canal, as Fig. 13 represents, can exert any active influence in favor of conception, or retain the spermatic fluid, if by chance entering it. Besides, if pregnancy should actually supervene, in spite of such conditions, the ovum would probably escape from the uterine cavity prema-

turely. And these expectations are confirmed by observation. Conception but rarely follows the operation as performed by Dr. Simpson; and, when it does, abortion is very likely to ensue. Dr. Gream[1] reported a case of this kind; Chrobak has had several cases, and I have myself known of six. Scanzoni admits that dysmenorrhœa is frequently relieved by this operation, but objects that sterility persists notwithstanding. Hegar and Kaltenbach had fair success in the former; but their results in the latter were "less brilliant." And Barnes remarks that the cure of sterility is not nearly so frequent as the cure of dysmenorrhœa (pp. 212, 213). I think the main facts on this point may be summarized as follows:

1. If the incisions close up, there is for a time an increased chance of conception; but the progressive induration and deformity of the external os—since Nature generally effects the closure very awkwardly—finally increase rather than diminish its original improbability. If the incision remain entirely unclosed, the sterility is generally confirmed.

2. The farther the operation stops short of the deep incision of Simpson, the better the prospect of curing the sterility. Hence, in some hands, it not very seldom succeeds, though performed by Simpson's or Greenhalgh's metrotome, simply because the cervical tissue is not divided deeply, as was done by the former. A distinguished obstetrician of this city informs me that he cured his first two patients of sterility by Simpson's metrotome, but has very seldom succeeded since, for the reason, I suppose, that he divided the cervical canal but very slightly at first, and became bolder from experience. Whatever of success Chrobak has had in the treatment of sterility by incision of the cervix, I attribute to his modifications of Simpson's method. He incised the internal os but three times in two hundred and fifty cases. He, however, varied his operations, and also resorted to other treatment, to such an extent in different cases that his statistics are of no value for my present purpose, except so far as they recognize the decided tendency to abortion if pregnancy afterward en-

[1] Dr. Sims, p. 170; from *Lancet*, April 8, 1865.

sues. There cannot, I think, be a reasonable doubt that Simpson's operation, performed on a woman in perfect health, would almost certainly render her sterile, unless the incisions closed up.

3. Sterility is more likely to be cured by the deep incision —as Dr. Barnes remarks: "the younger the patient and the more recent the stenosis, and before its complications are developed." I also add that more prompt and complete closure is likely to occur in such patients.

The last remote effect of this operation which I shall mention is, the eversion of the labia uteri, in case the vaginal portion has been cut through on both sides. I agree with Dr. Sims that this is a very grave objection, though his own operation will be seen to produce the same effect far more certainly and frequently.

Finally, in specifying the *uses* of Simpson's incision of the cervix, I cannot recommend it in the treatment of stenotic dysmenorrhœa or sterility. It frequently cures the former, and the latter very seldom. But it is unnecessarily severe and dangerous, and a safer operation may and should be substituted. Of the two metrotomes, I would not recommend Greenhalgh's in any uterine condition which occurs to me; but Simpson's will be found a very useful instrument for incising uterine fibroids projecting into the cervical canal or the uterine cavity, and still covered by the uterine mucous membrane.

II. Discission of the Cervix Uteri—*Sims's Operation.* This method of trachelotomy was first practised by Dr. Sims, in January, 1857. His operation consists:

1. Of a complete severing or discission[1] of the whole of the vaginal portion of the cervix, up to its vaginal attachment on both sides, by scissors; and—

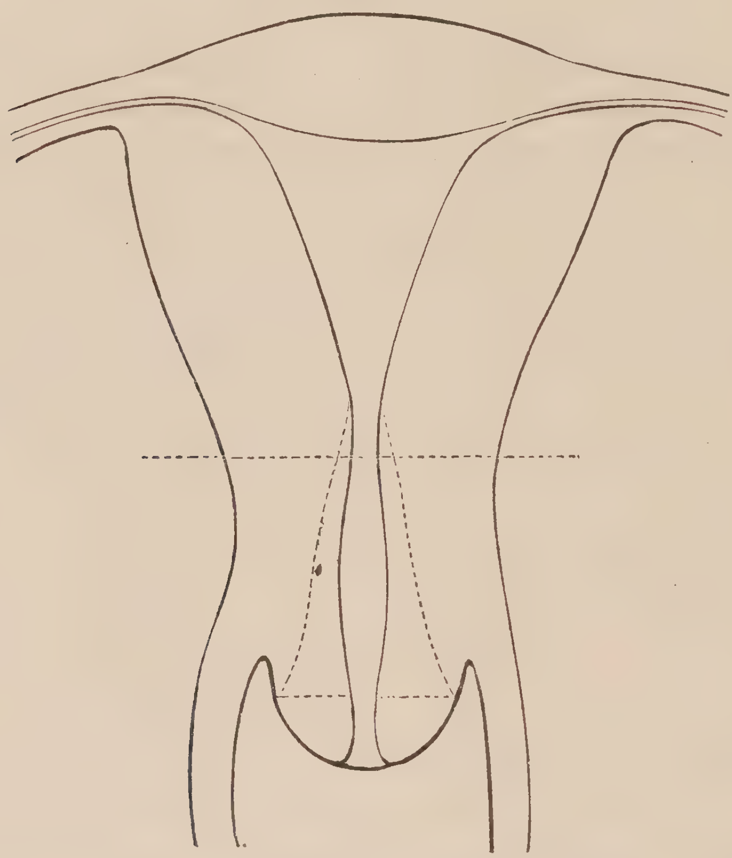
2. An incision of the whole canal above, and including the os internum, on both sides, by a narrow, razor-pointed knife.

Fig. 5 shows the manner of performing the operation, and Fig. 12 the shape of the uterine cavity after it. Since

[1] From *discindo*, to sever, to cut apart.

the former is taken from Dr. Sims's work on "Uterine Surgery," it may be regarded as expressing his average inten-

Fig. 5.



Manner of performing Sims's Operation; Sims's Surgery, p. 172.

tion in respect to the extent of the second step in the operation; for it is impossible for any one to know precisely how much he has cut into the canal, or to certainly cut the two sides precisely alike. From the tendency to hæmorrhage after this operation, I think the cuts at the internal os are usually deeper than here shown. The cervix is also only $\frac{3}{4}$ of the normal size.

Dr. Sims claims that his operation does not divide the entire cervix to so great an extent as the metrotome of Greenhalgh. But I shall show that the division is still much in excess of what is required, while it is attended by as great a change in the uterine cavity, and the same dangers in greater degree, and is followed by as undesirable remote results.

1. It is shown by Dr. Sims's diagram (Fig. 5) that the first

step in his operation practically shortens the cervical canal to an amount precisely equal to the length of the vaginal portion, or on an average nearly half an inch; for its discission on both sides practically annihilates it as completely as if it had been entirely removed by amputation. The relative length and shape of the whole uterine cavity, including the cervical canal, after this operation, as compared with its normal condition, are shown by Figs. 12 and 10. It has assumed the form of a flattened hour-glass, and the cervical canal is no longer fusiform, has lost nearly one-third of its length, and become as capacious as the cavity of the corpus.

2. The *dangers* of the operation are the same, but considerably exaggerated, as those of Simpson's operation, viz.: hæmorrhage, pelvic cellulitis, and septic peritonitis.

The danger of hæmorrhage from the arteries around the os internum (Figs. 3 and 4) is not materially greater or less than in Simpson's operation. A bold or a careless operator will cut as far with the knife as with the metrotome, and still more at random and less symmetrically, while a timid cutter will probably go less deep with the knife without a less probability of dividing the arteries. But, in another respect, the danger of hæmorrhage is much greater in Sims's operation, viz.: from the fact that the whole vaginal portion is completely severed, on both sides, up to the vaginal attachment. An alarming bleeding is also liable to occur suddenly at any time within the four or five days after the operation, though it was not very profuse during it; and special precautions should be taken both to prevent it and to arrest it if it takes place. It is usually arrested at the time of the operation by the application within the incision of cotton dipped in the persulphate of iron, and by the vaginal tampon. But a surgeon, who had performed this operation very many times, assured me that he never undertook it unless he could have his patient so situated that she could be visited at the shortest notice by himself or a competent assistant, for the next four or five days. But, in spite of these precautions, fatal hæmorrhage has ensued.

The risk of pelvic cellulitis and septic peritonitis is also

greater in discission of the cervix than in Simpson's operation. For, since the cut extends *through* the medullary portion of the cervix on both sides to the vaginal junction, in addition to the incision in the canal above, a greater surface and facility are afforded for the absorption of any septic agent.

I fully agree, therefore, with Dr. Thomas, that "had all the fatal cases which have occurred in consequence of this operation been published, as they should have been, the list would be a startling one" (p. 414). He had known of five cases, and had rumors of others. I could myself add as many more.

It would seem hardly necessary to state that an operation, liable to jeopardize a patient in the ways just specified, should never be performed without an imperative necessity. Dr. Skinner, indeed, maintains that the vaginal portion should never be cut through.[1] This, however, like the preceding procedure, has been but too often practised apparently as a matter of fashion, or of the force of habit; for I can give no more charitable explanation of several cases which have fallen under my observation. In one instance, no reason had been assigned for the discission. It could not have been done, I think, for dysmenorrhœa, for the patient had passed the menopause at least ten years before; and the age of fifty-five to sixty years would seem to exonerate her, also a maiden, from an operation for sterility. In another instance the patient had a short time previously been under my care, and I knew she had had no stenosis of the cervical canal, nor dysmenorrhœa from any cause. And the operation was hardly demanded for sterility, since she had been married but four or five months. I ascribed it to the force of habit, or, possibly, to a reckless "*besoin d'opérer*," which sometimes possesses a mere surgeon.

3. The *remote effects*, curative and otherwise, of discission of the cervix are in general the same as those already specified, of Simpson's operation. That is, it generally relieves, and often cures, dysmenorrhœa, when depending on stenosis; while

[1] "Liverpool Medical and Surgical Reports," 1865.

as generally it does not cure sterility, and does predispose to abortion if pregnancy occurs. Indeed, in these last two respects, it is more objectionable than Simpson's method; for —(1) the severance of the cervix on both sides not only at once destroys all contractile force in favor of conception as completely as if it had been entirely removed, but (2) the two pendulous flaps also act as valves to prevent the entrance of the spermatic fluid into the cervical canal. Besides, (3) the retentive power of the proper uterine cavity is diminished by the practical ablation of the vaginal portion and the change in its form as just explained. I should, therefore, apply to this operation also, so far as its value in the treatment of sterility is concerned, the three propositions already applied to Simpson's incision (pp. 416, 417).

Sometimes, however, the flaps become everted, and present the same appearance as if the cervix had been ruptured to the vaginal attachment in parturition; which latter condition, like the high amputation of the cervix, is well known to produce sterility in most cases, and to predispose to miscarriage, should pregnancy, notwithstanding, occur. This eversion, mentioned by Dr. Sims, as an objection to Simpson's operation, occurs far more frequently in his own; since, in the latter, the entire cervix is *always intentionally* severed, while in the former it is only seldom and accidentally so. All gynæcologists at the present day recognize the importance of the reclosure of the cervix ruptured in parturition. I maintain that the same should be done here, and the sooner after the discission the better. In other words, if a surgeon finds he has severed the cervix on both sides to the vaginal junction in the treatment of dysmenorrhœa or sterility, he should *at once close it up* again by the proper operation. After a few months the flaps become atrophied to such a degree that it will be difficult, and perhaps impossible, to restore the external os and the cervical canal to their normal shape and dimensions.

I have already alluded to the tendency to closure of the incisions after this operation; and which is, comparatively at least, a fortunate event, as increasing temporarily, if not per-

manently, the chances of conception. But Nature generally accomplishes this in a very imperfect manner. The two incisions are scarcely ever completely closed; and sometimes the attempt is made only on one side, in which case the contractility of the vaginal portion is still completely lost. If the two incisions are unequally closed, the form of the external os is of course abnormal; and, equally as if no closure had occurred, is unfavorable for conception. And, finally, if both incisions are quite closed, just as if only partially so, the cicatricial tissue gradually becomes indurated and contracted; so that, in the end, a return of the stenosis and of the dysmenorrhœa, in an aggravated form, may be the consequence.

Such being the merits of discission in the treatment of dysmenorrhœa and sterility, I should next speak of it as a remedy for anteflexion.

Surprising as it may appear to one familiar with the anatomy of the uterus, mere discission of the cervix to the vaginal attachment was formerly practised for its flexions.[1] I was told, several years ago, by one who had operated many times for these affections, that complete discission of the vaginal portion actually did, in his experience, return the body of the uterus to its normal position!! This is precisely as logical a procedure as dividing the sphincter ani for stenosis of the sigmoid flexure, or slitting the meatus urinarius for a stricture of the membranous portion of the urethra. On expressing my skepticism, and inquiring how a mere cut could affect the reposition, when it did not reach to within at least three-quarters of an inch of the point of flexion, he replied that he could not explain, but was sure of the fact. Of course the operation merely relieved temporarily the congestion of the displaced uterus, and removed some of the symptoms, by the free hæmorrhage it occasioned,[2] and which half a dozen

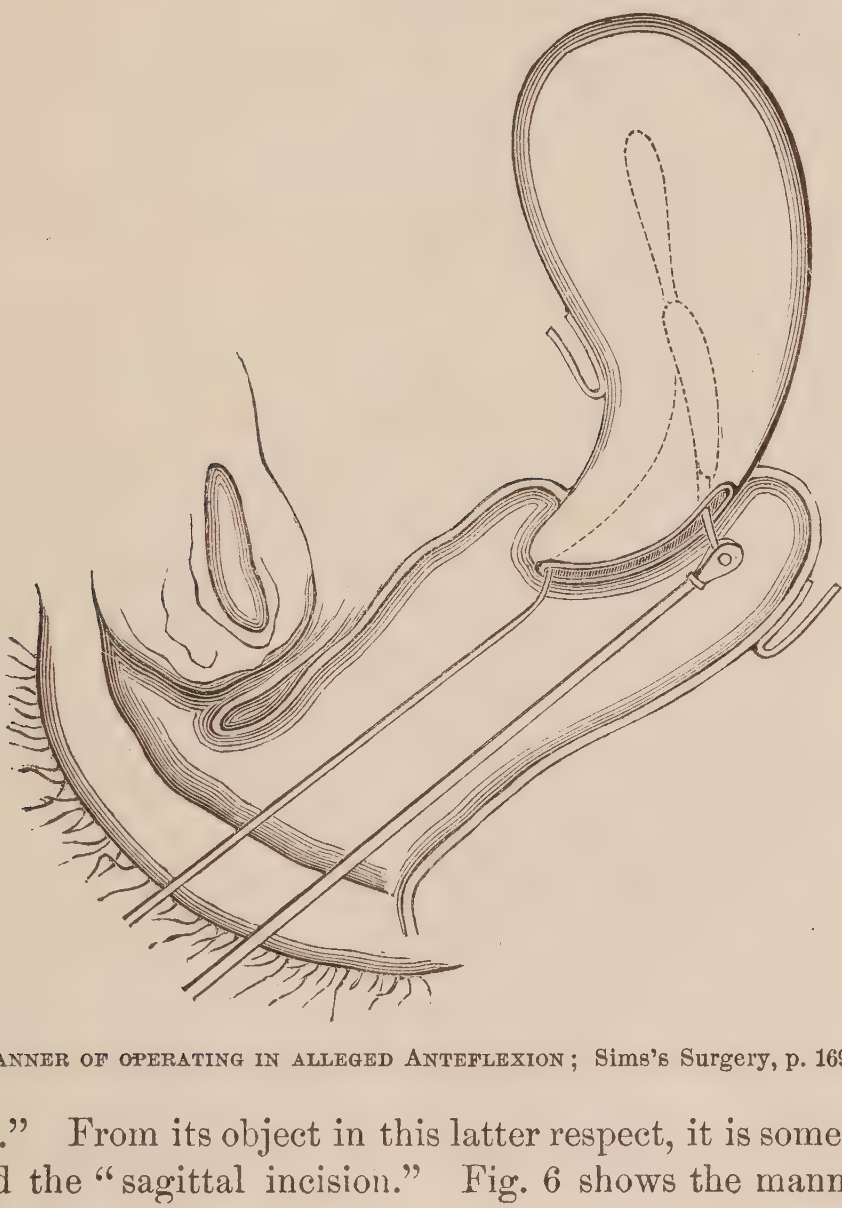
[1] I am, of course, not here speaking of flexions of the cervix alone; but of anteflexion and retroflexion of the uterus.

[2] Dr. Barnes, indeed, speaks of the bilateral division of the cervix as a very effectual method of local depletion in the treatment of anteversion (p. 583). But the hæmorrhage is not the object aimed at by him in this condition, but merely an accident that may do good in that way also.

leeches would have done far more safely. I allude to this application of discission of the vaginal portion merely as a matter of its history.

Next, we find, the following advance in the treatment of anteflexion was proposed: First, the vaginal portion was divided posteriorly up to the vaginal attachment by scissors; and then a narrow razor-shaped knife was passed up the cervical canal and through the internal os; and an incision made anteriorly above and posteriorly below, so as "to bring the incision of the cervical canal into a line with the uterine

Fig. 6.



Manner of operating in alleged Anteflexion; Sims's Surgery, p. 169.

cavity." From its object in this latter respect, it is sometimes termed the "sagittal incision." Fig. 6 shows the manner of

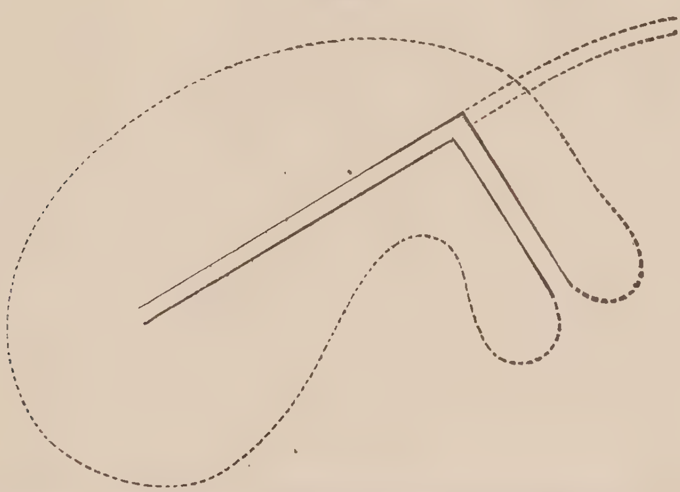
making the incision, and the straightening alleged to be effected thereby; it being the now stereotyped diagram used by most writers on this subject since first published by Dr. Sims in 1866.

Even if this operation could accomplish what it promises in anteflexion, there would be an insuperable *a priori* objection to it, since it ignores and contravenes the first indication in all flexions of the uterus, viz.: to return the corpus uteri first of all—unless inflammation, preventing the necessary manipulations, coexists—to its normal position; for this procedure not only leaves the corpus in its abnormal position, but aims to keep it there permanently by producing in the cervical canal another abnormal condition still more serious (and it is often incurable) than the original displacement. Hence Dr. Thomas proposes it only in case the anteflexion is irreducible. It will, however, be seen that this operation does not at all effect what it proposes in anteflexion, and simply because in that condition of the uterus it is impossible.

Since the axis of the uterine cavity and the cervical canal meet at the internal os at an angle of 165 degrees, it will be seen by referring to any diagram representing an antero-posterior section on the median line, of the normal uterus, that an incision made into the cervical canal in a straight line, in continuity with the axis of the uterine cavity, would fall barely below the vaginal attachment, and within the vagina, just as Dr. Sims has represented it; or, in other words, that Dr. Sims's sagittal incision can really be made only when the corpus uteri itself still remains in its normal position. If the corpus be bent forward at a right angle, the cervix would have to be split through posteriorly to the internal os, and into the peritoneal cavity, to carry out the idea; and, in the extreme of anteflexion, the incision must aim toward a point somewhere in or above the lumbar region. Indeed, the total inapplicability of the sagittal incision to anteflexion is admitted by Dr. Sims himself when rightly interpreted. For, after describing his case as "curvature, with elongation of the vaginal portion accompanying anteflexion" (p. 166), and show-

ing how the canal of the cervix is "made to run in a straight line from the cavity of the uterus to the terminus of the incision," by two diagrams (pp. 167, 169), he adds, "The operation is wholly inapplicable except in just such cases as the one above described" (p. 168). But neither of these figures represents anteflexion at all; but simply curvature of the cervix alone, without any abnormal position of the corpus above it. The same remark is also applicable to the diagram (Fig. 7) intended to represent the sagittal incision in anteflexion, in Dr. Thomas's valuable work (p. 412). The preceding figure (6),

Fig. 7.



Sagittal Incision in alleged Anteflexion; Thomas, p. 412.

copied by most writers on anteflexion since 1866, has misled hundreds of operators, who have never challenged its accuracy, in actual cases of anteflexion; and it is time it is generally understood to be imaginary and fallacious.

The application of the sagittal incision (posterior discission) in flexion and curvature of the cervix alone, in both of which it is very often, but by no means always, required, is well shown by Fig. 8, which is also copied from Dr. Sims's work (p. 167).

In specifying the *uses* of Sims's operation, I should, on the before-mentioned grounds, reject it in the treatment of sterility,[1] and also object to it in cases of dysmenorrhœa, as

[1] See the results of Dr. G. Braun, of Vienna (*Wien. med. Wochenschrift*, 1869); and of Chrobak, *Weibliche Sterilität*, Wien, 1876.

unnecessarily severe and dangerous, in comparison with another operation, which I shall advocate; and I know of no

Fig. 8.

Sagittal Incision in Flexion of the Cervix; Sims, p. 167.

other condition in which it should be recommended. But bilateral discission alone, up to the vaginal attachment, is justifiable in some cases already described on a preceding page, of uterine fibroids, as a substitute for incision with Simpson's metrotome, to aid their descent into the vagina. But, in such cases, the incisions should be closed with sutures as soon as the fibroid is removed. In these cases also an incision on one side only is frequently sufficient. Bilateral discission, to a limited extent ($\frac{1}{4}$ inch, or even less), as practised by Dr. Barnes, is also applicable to some very rare cases of conical cervix. Finally, it has been shown that complete posterior discission is required in many cases of flexion and curvature of the cervix alone.

Having thus stated my own convictions respecting the real value of Simpson's and of Sims's operation, and incidentally also of mere bilateral and posterior discission, I close this part of my paper with some quotations from the latest writers on this subject, stating especially the points upon which they are not in accord with myself.

Schroeder,[1] who practises Sims's operation, says, "Inflammation of the uterus and cellular tissue does not occur if the operation has been performed with clean instruments; the hæmorrhage may, however, be considerable, and occasionally serious" (p. 71).

[1] Diseases of the Female Sexual Organs.

Dr. Thomas thinks Sims's operation, *per se*, is not attended by great danger. It is the performance of it when pelvic peritonitis exists in chronic form, that has caused it to produce such bad results" (p. 414).

Hegar and Kaltenbach regard the sagittal incision as more dangerous than bilateral discission, the latter being less dangerous than heretofore supposed. But they really follow neither Simpson nor Sims, never using scissors, except as preliminary to amputation of the cervix; but merely a knife, cutting the internal os two to three millimètres (one-twelfth to one-eighth of an inch), and the external os, one and a half centimètres (three-fifths of an inch.) Thus, in fact, they do not perform discission at all, and may well find their own procedure not so dangerous as actual discission has "heretofore been supposed to be." Still they lost two patients in 150 cases. Their results were good in dysmenorrhœa, but less brilliant in sterility.

Dr. Hewitt prefers Sims's operation; but with the important modification of cutting but very little at the internal os. He thinks that "the incision treatment will be restricted within narrower limits than was the fashion three or four years ago" (p. 399).

Dr. Barnes's testimony is seemingly somewhat conflicting in different portions of his work; but he is sometimes speaking of Sims's operation, and sometimes of incision of the cervix, as practised by himself. He says: "Sims's is an effectual operation, but unnecessarily severe" (p. 214). He uses scissors, or Simpson's metrotome, but never cuts the internal os at all. He thinks it generally quite safe to divide all the vaginal portion of the cervix (p. 210), the risk of the operation itself being infinitely small, if proper precautions be taken. From the want of them, bleeding and peritonitis are not uncommon results. He has seen several cases of chronic pelvic cellulitis arising in this manner; and some fatal cases of bleeding are known to have occurred (p. 206). Further on, however, he accounts for the safety of the operation performed by himself, when he says: "it is enough to make a good transverse slit or os tincæ, which shall give free communication between

the cavity of the cervix and the vagina. The part thus divided is not very vascular, and it is rare that any bleeding of importance occurs" (p. 216). He, however, keeps his patient four days in bed, and in her room for a week (p. 210). He advises bilateral incision (but not discission) in some cases of primary retroflexion, but merely to remove the stenosis of the os externum, which usually attends it (p. 612). Finally, he indorses Sims's operation in anteflexion (p. 594).

From the preceding facts I deduce the following conclusions:

I. The deep incision of the cervix throughout, and complete bilateral discission of the vaginal portion with deep incision above, are alike frequently attended by certain immediate dangers, and, not seldom, productive of certain serious remote consequences; viz., profuse and sometimes fatal hæmorrhage, pelvic cellulitis, septic peritonitis (usually fatal), sterility, if not previously existing, and a tendency to miscarriage.

II. Those risks and effects are all due to the extensive division of the walls of the cervix, and to the consequent enlargement of the cervical canal; and the sole compensation for all of them which can be calculated upon is, the relief, and very often the cure, of stenotic dysmenorrhœa.

It therefore becomes a question of very great practical importance whether the amount of cutting may not be so far diminished as to avoid all these risks, and at the same time be sufficient for the cure of stenotic sterility and dysmenorrhœa. But another inquiry antecedent to this is: how large a calibre of the cervical canal is actually required for the relief of these two conditions; and a reply sufficiently definite for all practical purposes is not so difficult as might appear.

In the *imparous* woman, the narrowest point of the cervical canal, viz., the internal os, is, when opened by the passage of the menstrual fluid, an ellipse, whose conjugate and transverse diameters average respectively one-sixth and one-eighth of an inch; its area corresponding very nearly[1] with that of

[1] The circle is smaller than the ellipse, in the proportion of 144 to 147.

a circle one-seventh inch in diameter. The external os, also elliptical when moderately dilated, has diameters averaging one-fourth and one-sixth of an inch. It thus has an area exactly twice that of the internal os, and equalling that of a circle one-fifth inch in diameter.[1] The larger size of the external os doubtless has a special reference to conception, and favors the entrance of the spermatic fluid into the cervical canal. It has no special influence against dysmenorrhœa, since the menstrual fluid, after having passed through the internal os into the cervical canal, would pass just as easily from the latter, through an opening of the same dimensions, into the vagina. Hence, we not very seldom see imparous women with the external os no larger than a "pin-hole," and who, nevertheless, do not suffer from dysmenorrhœa, though, as a rule, they are sterile. But if the lining membrane of the canal becomes thicker, from congestion, or some other cause, such patients suffer at once from stenosis at the external os.

In the *parous* woman, the size of the external os varies within quite extensive limits, since it is exposed to so many of the accidents of parturition, while the internal os is more nearly uniform.

I have deemed it desirable to ascertain the lowest average diameter of the two ora uteri in parous women, who are neither sterile nor have dysmenorrhœa, as a rational standard for determining the extent of incision actually required for the removal of these two conditions when stenotic. And, after a good deal of observation in this direction, I find that the inner os presents nearly twice the area of that of the imparous woman ; in the majority of cases admitting a sound one-fifth of an inch in diameter—though, in a large minority, one from one-fifth to one-sixth of an inch only can be easily passed. I therefore regard a diameter of one-fifth of an inch as ample for the removal of sterility and dysmenorrhœa. I find the external os admits a dilator one-fifth of an inch in diameter and upward—in some cases as high as one-fourth or even three-tenths of an inch—but, as a rule, I think one-fourth of

[1] Circle to ellipse as 72 to 75.

an inch sufficient for the purpose. It is of course to be understood that no narrowing of the canal exists between the two ora. Since, however, there may be some degree of stenosis for the menstrual fluid, while not for the sound, it is sometimes judicious (and especially if congestion of the cervical lining membrane coexists) to increase the dimensions just named, by the use of a dilator of the next larger size. I do not assert that the preceding dimensions are always required in the treatment of stenotic sterility and dysmenorrhœa, for they are not, nor that they are never to be exceeded, but that in almost all cases they will be found sufficient.

Should this precise specification of dimensions seem too minute for practical purposes, we must remember that dimension cannot here have a less important relation to function than elsewhere; and that enlarging the internal os to the diameter of half an inch, as is often done by the deep incision, is, as has been seen, like permanently dilating the urethra (if it could be done) to the size of the small intestine. And the importance of making an incision of the internal os, with a precise intention, and a precise knowledge of the mode of accomplishing what is intended, may be understood, when I state that if the circle representing its area in the imparous woman be increased equivalently to surrounding it by a ring only one-thirty-fifth of an inch wide, its area is increased as forty-nine to twenty-five, or almost exactly doubled. Or if an incision be made on each side of it to the extent of half a line (one-twenty-fourth of an inch), and it then be dilated to a circle, it is increased two and a half times. And if the cut should extend one line to the right and the left, or the added ring were one-twelfth of an inch wide, the area would be increased more than four times and a half. This last increase is far more, in my experience, than is ever required in stenotic sterility and dysmenorrhœa.

III.—Superficial Trachelotomy—*My own Operation.*

Desiring to restrict the operation of trachelotomy in the treatment of stenotic sterility and dysmenorrhœa within the limits actually required, I, some ten years ago, devised and

brought before the New York Obstetrical Society[1] a series of five steel cervical dilators, to be used instead of incision, where the stenosis is slight and the cervix is normally soft and pliable. These, in shape and size, have a precise reference to the dimensions of the cervical canal, and especially of the two ora uteri, as already specified; and each is guarded by a bulb, so as to project through the internal os into the uterine cavity only about one-quarter of an inch.

But finding that almost all cases of stenosis of the cervical canal are relieved, more promptly, more permanently, and also with less pain, by incision, or this together with dilatation, than by any form of dilatation alone, I next endeavored to restrict the extent of the incision within the absolutely necessary limits, having determined them approximately by the preceding facts and calculations. To this end, I devised a new method, and an instrument for executing it, which I also laid before the New York Obstetrical Society about eight years since; but the former was so simple, bloodless, and unpretending, in comparison with the procedures of Simpson and Sims, that it excited but little interest. Meanwhile, however, it has been sufficiently tested, I think, by myself and my pupils in different parts of the country, to entitle it to a more general notice.

Since the superficial incision, as suggested by myself, has for its direct object merely the removal of stenosis of the cervical canal, and is therefore proposed for the treatment of stenotic dysmenorrhœa and sterility only, it is previously to be decided whether stenosis actually exists. And the following propositions will aid in settling this question; it being understood that the exploration is to be made at least four days after, and at least three days before, the catamenial flow.

A.—*Respecting Stenosis of the Internal Os.*

1. If a sound one-fifth of an inch in diameter passes easily through the cervical canal, there is no stenosis at the internal os, and no incision is there required. This is the size, therefore, of my large sound.

[1] Also described in the New York Medical Journal, July, 1870, p. 478.

2. If a sound one-sixth of an inch in diameter be easily passed, as above, there is no absolute, though there may be relative stenosis of the internal os; i. e., there may be stenosis for the passage of a fluid, though not of the sound; and an incision to make it one-fifth of an inch may be required, but not unless the symptoms indicate it.

3. If the sound easily passed be but one-seventh of an inch in diameter, and there are no symptoms of stenosis, no incision of the internal os is required. This is the normal size in the imparous woman, and the average size of Simpson's sound.

4. If a sound but one-eighth of an inch in diameter cannot be passed through the internal os, there is either stenosis or, what is very much more probable, one of the flexions. Prove, therefore, that there is no flexion in this and every case in which a sound of any size does not traverse the internal os before operating for stenosis. I consider an internal os of one-eighth of an inch or less to be stenotic. Chrobak's highest limit for stenosis of the internal os is one-tenth of an inch (two and a half millimètres).

B.—*Respecting Stenosis of the External Os.*

5. On the other hand, there is no stenosis of the *external os* if a sound one-fifth of an inch in diameter easily traverses it. If there be congestion of the lining membrane, however, there may be stenosis, practically, in respect to conception; and the operation somewhat enlarging it (to one-quarter of an inch or more) may be required.

6. If the external os will not easily admit a sound one-sixth of an inch in diameter, there is probably stenosis in respect to conception, and the operation is required. If not more than one-seventh of an inch, the operation will also probably be required for dysmenorrhœa.

7. In case of operation, the whole cervical canal must be made still to retain the normal fusiform shape as far as possible.

I. My *method* consists in incising the internal os, if the stenosis exist at that part—and the external, if at the latter—

to such an extent as to give to each its precise average dimensions in the parous woman, neither more nor less, and, of course, also overcoming any other point of stenosis existing anywhere else in the cervical canal. In cases complicated with congestion, however, I have shown that a slightly larger opening may be required, and, therefore, that the limits may extend beyond one-fifth of an inch to nearly a quarter of an inch in the case of the internal os, and to three-tenths of an inch, and possibly more, of the external.

I do not, therefore, incise the internal or the external os to a given depth in all cases, but, taking them as I find them, cut just enough to give them their average normal size in the parous uterus. This is seldom one-half a line and often not more than one-third of a line for the internal os, and not more than a line for the external. But, of course, there is far more variation in the latter. If the internal os admits a sound of but one-eighth of an inch in diameter, a cut on each side of nearly half a line (but three-eightieths of an inch) is required; and, if but one-tenth of an inch in diameter, it must be one-twentieth of an inch deep on each side. The incisions are of precisely the same depth on each of the two sides.

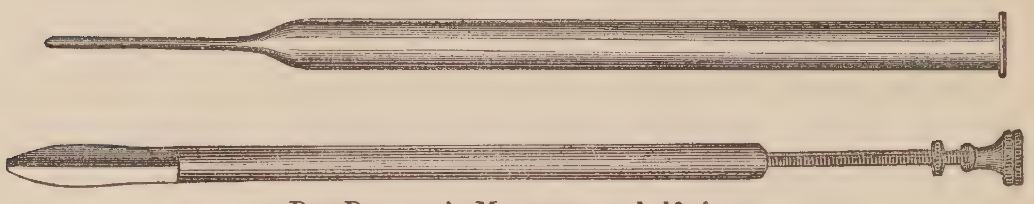
Since the lining membrane at the internal os is at most one-twenty-fifth of an inch thick, it is seen that I generally do not cut nearly through it. Indeed, when the os is but one-eighth of an inch wide, I cut almost through the membrane; and, when one-tenth of an inch, I divide it and one-hundredth of an inch of the tissue beneath it.[1]

II. The *instrument* devised to secure this effect consists of a flattened tube, containing a blade. The former is eight inches long and seven-sixteenths of an inch wide, except its terminal one inch and three-quarters, which has a width of but one-eighth of an inch, as shown in Fig. 9. This portion is made curved by some instrument-makers, which is not an improvement. The blade is of such a width as to slide accu-

[1] The details of all the preceding calculations are properly omitted here, as a slight acquaintance with mathematics will enable the reader to verify them.

rately within the tube, having a nut and a screw attached to its proximal extremity to gauge the extent of its passage into the cervical canal, and a blunt point and lateral cutting

FIG. 9.



DR. PEASLEE'S METROTOME, half size.

edges for an inch and five-eighths at the distal end. There are two blades for each instrument, the cutting portion of one being a quarter of an inch wide, and of the other three-sixteenths of an inch. If the stenosis is confined to the internal os, the narrower blade alone is used. If both ora are contracted, the wider instrument is passed through the external os, and the other blade then introduced and the inner os incised by it; and, in cases of decided congestion, the wider blade alone is sometimes used for both ora. In this case, a sound one-fifth of an inch in diameter is easily passed through the inner os; while, if the smaller blade had been used, considerable force would be required to carry it through.

In hospital practice, I place the patient upon the side, use the duck-bill speculum, hold the cervix by means of a uterine tenaculum, pass the tube into the canal up to the shoulder, and, therefore, one-quarter of an inch into the uterine cavity through the internal os, when the blade, previously gauged, is introduced into the tube, and carried up the cervical canal as far as is required to overcome the stenosis. My large sound (No. 10, American scale), or, still better, the conical dilator of the proper size, is then passed up the canal, and the operation is completed. In private practice, I generally place the patient on the back, and pass the tube into the cervical canal precisely as I would Simpson's sound, and then pass the blade through it, as just described.

If the external os is too narrow for the admission of the extremity of my instrument, it may be enlarged by the in-

troduction—generally one-eighth to one-quarter of an inch is far enough—of a narrow-pointed bistoury. I have not found the internal os too narrow to receive it, except in cases of flexion, or previous traumatic injury of the cervix.

The changes in the whole uterine cavity from this operation are shown by Fig. 11. Respecting its dangers I have but little to communicate. The hæmorrhage following it seldom exceeds one or two drachms, and never requires any special attention. The pain is very slight and momentary, and no anæsthetic is ever required. The medullary structure of the cervix never being cut into, pelvic cellulitis and peritonitis do not ensue. The only exceptions to this statement in nearly three hundred cases are, one case in private practice, in which some febrile reaction and uterine tenderness ensued, which subsided entirely, without cellulitis, in four days; and two cases, in the Woman's Hospital, of slight cellulitis. But both the latter were patients who were known to have had cellulitis a short time previously, and I was obliged, by some peculiar circumstances, to operate sooner than I otherwise would have done. The final results were precisely as desired in each of these three cases. Otherwise I have never had any unpleasant symptoms follow the operation; and the only precautions taken are to keep the patient two days, and sometimes three days, in bed, and not allow her to walk out under a week. I use the dilator every second day after the operation for a week, and two or three times more once a week. I have very often performed the operation at my office on residents of the city, and sent the patient home to bed after half an hour's rest, and have never had to regret it. I decline to operate within four days after or six days before the catamenial period.

I claim for the method just described the following recommendations in the treatment of stenotic sterility and dysmenorrhœa:

I. It aims to restore the normal dimensions as existing in the parous woman throughout the cervical canal—nothing more and nothing less—unless where a slight exaggeration of size is required on account of coexisting congestion.

II. It effects this object definitely and with certainty, and with incisions exactly symmetrical, or equal on the two sides.

III. It gives no danger from hæmorrhage, since the arteries nearest the internal os, if that is to be divided, are never reached, and the whole thickness of the lining membrane even is generally not divided; and there are no arteries within the portion divided at the external os.

IV. There is no danger of pelvic cellulitis, except in those patients in whom the least operative interference with the cervix, or the use of the sound or of a sponge-tent, will produce it. I consider the operation less dangerous in this respect than the last mentioned.

V. There is no danger of septic peritonitis, since the medullary substance is not reached by the incision.

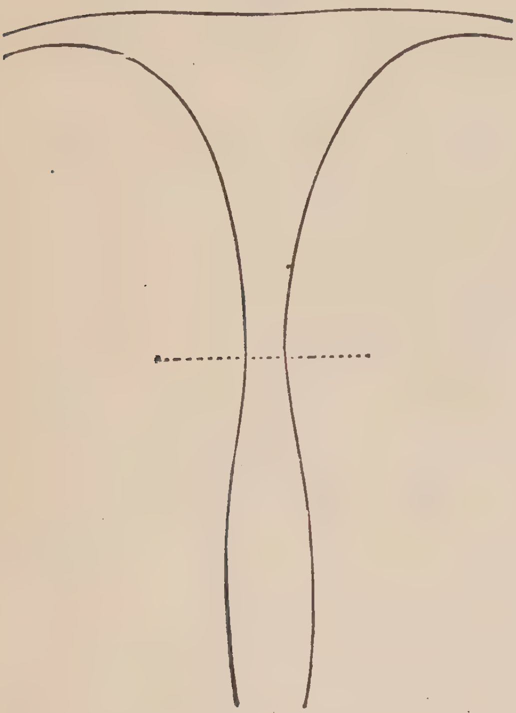
VI. It does not produce sterility or tendency to abortion by mutilating the cervical canal. The changes it produces in the latter, as compared with those from the operations of Simpson and Sims, are shown by Figs. 10, 11, 12, and 13.

VII. It removes stenosis perfectly, and in most cases permanently, since there is very little tendency to closure of the slight incision made. I have had to repeat the operation only twice in my practice, except in cases in which there was cicatricial tissue to be divided, as after imperfect and partial closure following rupture of the cervix in parturition, or ensuing after Simpson's or Sims's operations. Here the operation will usually have to be repeated in a year or two, unless pregnancy should occur, an event not to be expected in such cases, as we have seen.

Finally, then, since my experience has shown that a diameter of one-fifth of an inch for the internal os, and of one-quarter to three-tenths of an inch for the external os, is sufficient in the treatment of stenotic sterility and dysmenorrhœa, I suggest the disuse of Simpson's and Sims's operations in the treatment of these conditions, and the substitution of a milder, safer, and more efficacious method, of which, perhaps, my own is, however, only the forerunner. At least, further

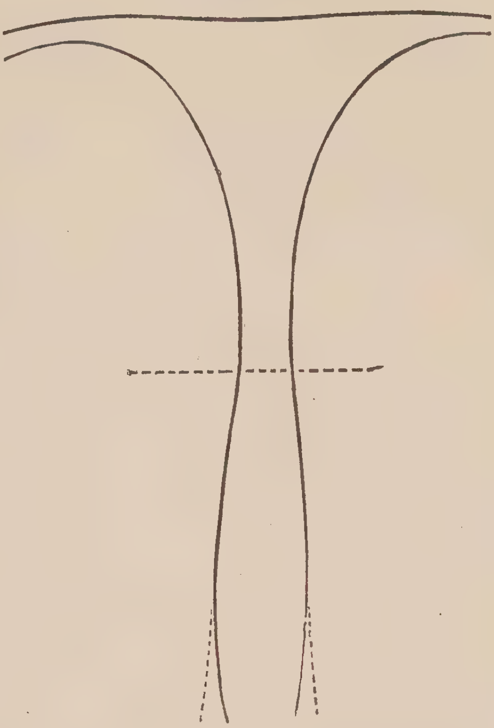
experience in the line I have indicated will doubtless afford still more accurate conclusions.

Fig. 10.



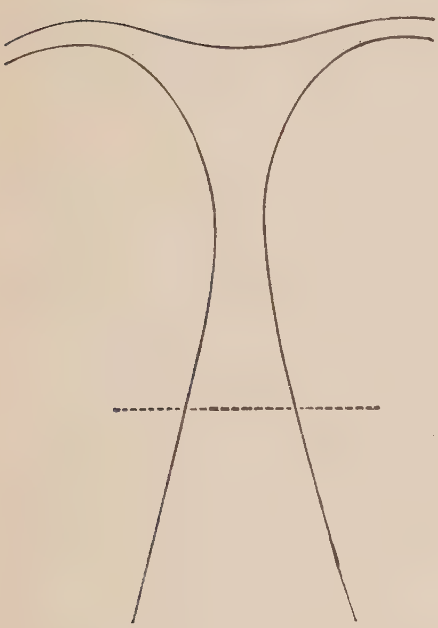
Normal Uterine Cavity.

Fig. 11.



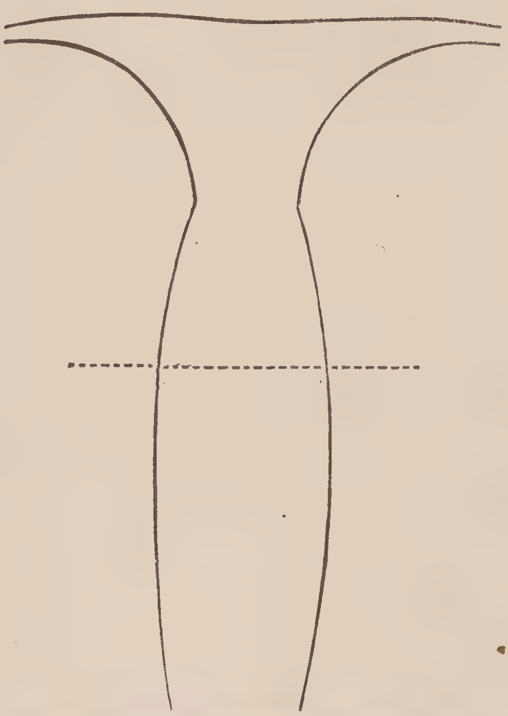
Ditto, as modified by Peaslee's Method.

Fig. 12.



Uterine Cavity after Sims's Operation.

Fig. 13.



Ditto, after Simpson's Operation.

CHOREA: ITS CAUSE AND TREATMENT.

By GEORGE T. STEVENS, M. D., Albany, N. Y.

Read by Invitation, June 15, 1876.

Notwithstanding the labors and careful researches of many observers who have brought great patience, experience, and skill to the investigation of the subject of chorea, its nature and cause have eluded their search, and chorea has remained a scientific mystery to the present day.

In an able paper published in the London *Lancet*, in May, 1873, Dr. Anstie justly remarks: "The causation and pathology of chorea form one of the most puzzling subjects which medical inquiry has ever encountered;" and, if one compares the knowledge of the cause, treatment or pathology of the disease possessed by the medical profession to-day with that held a century ago, he will find that little advance has been made.

Commencing with the treatment, we find evidences of the uncertainty of our knowledge in the multitude of remedies. Glancing over the recent literature of the subject, we find that steel, antimony, zinc, arsenic, chloral, and strychnine, Indian hemp, apomorphia, Calabar bean, and electricity, rest in bed, and aloëtic purges, with a long list of other remedies, have their advocates, while of the greatest number of writers each seems to think that every remedy, except his own favorite, is inefficacious.

The truth is that the tendency is generally to a *quasi* recovery, and the physician who administers tonics and sedatives encourages this tendency; and, when the patient has

made the so-called recovery, the medical attendant is prone to imagine that his peculiar treatment has wrought the cure. One of the most unfortunate facts about the lack of knowledge of chorea is that one which it appears to me has been almost entirely overlooked—that a very large proportion of the cases which are supposed to have recovered are in reality doomed to a life of mental weakness, stammering speech, deformed limbs, disease of the heart, or other serious and permanent injuries, and the proportion of deaths is far from being the most serious or important consideration.

If, now, we inquire what are acknowledged to be elements in the cause of the disease, or where is its seat, we find that nothing is known of the first and little of the latter.

Once it was supposed that chorea was dependent upon spinal irritation, but more than twenty years since Dr. Reynolds pretty clearly demonstrated that the phenomena of chorea were not such as to favor the view of a spinal origin. He further argued that the phenomena of the disease seemed to point to the view that its pathological seat is the sensori-motor ganglia at the base of the brain, the corpora striata and optic thalami. Dr. Broadbent, at a later period, added new arguments to this view, and still later Dr. Hughlings Jackson adopted, in a modified form, the same theory.

In the comparatively few cases in which *post-mortem* examinations have been made of persons who were suffering from chorea at the time, or very near the time of death, either no lesions have been found, or such as have possessed little significance, until within a few years the careful researches of Dr. Hughlings Jackson suggested to him the hypothesis that the proximate cause of chorea is embolism of some of the capillary vessels supplying the corpus striatum and neighboring convolutions.

This hypothesis has attracted great attention in the scientific world, yet, notwithstanding the great authority of its distinguished originator and the weight of his ingenious and elaborate arguments, many eminent and judicious observers feel that it is by no means of universal application. Thus the

question of the cause and the pathological seat of chorea is as much a scientific puzzle as ever.

In solution of this riddle which has so long taxed the ingenuity of the scientific world, I announce the following proposition, which I believe has never in any form or part been submitted to the medical profession by another:

Chorea is a functional disturbance of the nervous system, which may give rise to organic lesions and which arises from irritation dependent upon anomalous refraction of the eye, and in a very large proportion of cases to hypermetropia.

In thus boldly announcing before this learned society a radically new doctrine respecting a disease which has baffled men of the greatest skill and eminence in science, I am fully aware of the danger of my position and of my responsibility to maintain my proposition; but, in order that I may not assume more than I can hope to demonstrate, I desire at the outset to exclude from our discussion certain forms of disease which I have never seen, and which, if they are properly classed with chorea, may have, for their causes, influences entirely different from those which I believe to be active in the production of the disease as commonly known. Thus certain epidemic forms of mania and delusion have spread through certain localities affecting similarly large numbers of persons, leading them to shake and to dance, to jump and to exhibit other epidemic manifestations of nervous derangement. For the purpose of this discussion such epidemic forms of frenzy are excluded.

In support of the proposition I have announced, I shall first offer some of the clinical experiences which led me gradually, and almost unconsciously to myself, to a belief in the intimate relations between chorea and erroneous refraction; then some observations which I have made with a view of testing the correctness of this theory, and finally I shall endeavor to show that this is a most reasonable and natural conclusion.

The first instance in which my attention was specially directed to the coexistence of chorea and defective refraction was in the case of a little boy, who, five years ago, was pre-

sented at one of my clinical lectures at the Albany Medical College. The boy had suddenly suffered a great loss of vision, and on ophthalmoscopic examination it was found he had apoplexy of the retina in both eyes, and that he had also a high degree of hypermetropia. It was also discovered that he was suffering severely from chorea. I was at the time disposed to regard the retinal apoplexy as in some way the result of the choreic affection, but considered the hypermetropia as a coincidence of no special significance so far as the more serious feature in the case was concerned.

Within a few weeks after this incident, a bright girl, nine years of age, was brought to me by her father, from Vermont. She had suffered pain over the eyes, on account of which, some months previously, she had been removed from school. About the same time she had been taken with chorea. I found the child suffering from a hypermetropia of $\frac{1}{10}$, and prescribed glasses to meet this indication. The child, who was greatly delighted with the improved sight, was left under my charge for about three weeks, at the expiration of which time she returned home, every symptom of chorea having disappeared. Indeed, from the time the glasses were supplied a great improvement was visible.

Several other cases came under my observation from time to time, of which I will only specially mention two or three.

Mrs. V. H., of Massachusetts, brought to me her little son, seven years of age, she being concerned because the lad was getting a cast in his eyes. She wished to know if nothing could be done to prevent him from becoming permanently cross-eyed, and also if any relief could be afforded for his painfully nervous condition. While examining the boy's eyes by the ophthalmoscope, I observed that he was so restless that he could scarcely be kept on his stool. It was an undoubted case of chorea. The boy was, as I anticipated, hypermetropic, and convex glasses were prescribed with a view to prevent strabismus. Three months later the mother and boy called on me again. The child's appearance was greatly improved, and the mother reported that he was so much pleased with

his glasses that he would only permit her to remove them at night after he had gone to sleep. His nervous symptoms had entirely disappeared and the tendency to strabismus was also relieved.

After observing several cases similar to those already related, I came to regard the hypermetropia as the exciting cause of chorea when they were coincident, and was accustomed in such cases to assure the parents that the nervous symptoms would disappear on relieving the strain upon the accommodation ; but for two years past I have regarded the coincidence of chorea and anomalous refraction as unfailing, and have frequently expressed this view in my lectures to medical students.

During the summer of 1875, a lady, who was a neighbor, brought to my country residence her little girl, aged about seven years, to consult me in regard to her chorea. Having at hand neither trial glasses nor ophthalmoscope, I advised the mother to have the child's eyes examined as soon as possible, assuring her that she was oversighted, although I had no means at hand of proving it. I saw no more of the child, but was informed on good authority that she was brought to a distinguished ophthalmic surgeon in New York, who advised the mother that the child was hypermetropic and should wear glasses.

A woman in Troy, New York, brought her daughter, Carrie T., aged twelve, desiring me to prescribe for her chorea. She had observed no particular trouble about the eyes. I found hypermetropia $\frac{1}{30}$ and prescribed spectacles. I saw the girl several times afterward and had the satisfaction of seeing the symptoms of chorea rapidly disappear until the patient seemed entirely well.

I must not pass this portion of my subject without mention of two cases in which the refraction differed from that in the preceding cases.

Mr. F. A. R., of Minnesota, consulted me in September last in regard to his little daughter's eyes. She was a bright girl of ten years, with a defect in her speech, and appeared quite

restless and nervous. The trouble for which I was consulted was the presence of a number of small ulcers at the border of the cornea. Observing her restless appearance, I questioned the father in regard to the health of the child, and learned that for more than a year she had been kept from school on account of her nervous condition. She was fidgety and restless and complained that at times she suffered from headaches. Her father said that from time to time, but not always, she had twitchings of the head and of the face. She was clumsy with her hands and stumbled in walking. Her temper, by nature most amiable, had become irritable and sullen, and she would often give herself over to violent passion or to floods of tears on the most trivial provocation. On testing the refraction of the eyes I found astigmatism, which required for its correction a +36 cylindrical glass. After brief treatment for the ulcers of the cornea, resulting in their cure, the father returned with his child to his Western home. In the latter part of March of this year he presented the child again for treatment, she being again subject to corneal ulcers. I learned that from the time of leaving Albany the child's health had greatly improved; her nervous symptoms disappeared almost entirely, and so great was the improvement in her general health that it was a subject of universal congratulation among her friends. A month or two ago, however, her glasses were broken and she was without them for some time. The nervous symptoms began to return and the corneæ were again affected with ulcers.

A female operative in a knitting-mill in Cohoes, aged twenty, had chronic chorea, was rather feeble-minded, and had pain about the eyes. I found the right eye having astigmatism requiring a —24 cylindrical glass; with this, sight was only $\frac{5}{20}$. The left eye was emmetropic and sight good. There was insufficiency of the internal recti muscles.

Notwithstanding the fact that the relations between chorea and anomalous refraction have for a considerable time been recognized by me, and that I have for two years past spoken of this relation in my lectures, it was only in March last that

I instituted any course of systematic investigations with a view of testing more thoroughly the truth of the hypothesis which had, up to that time, grown upon me almost unconsciously. On turning over the pages of my case-books, I found that within five years I had observed thirteen cases of chorea, in all of which hypermetropia existed, except in two instances in which there was astigmatism.[1] Several of these cases had experienced the greatest relief from the use of appropriate glasses. Indeed, in no instance in which glasses had been used had there been a failure to relieve the nervous troubles.

I now sought to find the state of the refraction in eyes of persons who were suffering from, or who had suffered from, undoubted attacks of chorea.

The first case in this series of observations was that of a young girl, E. C., aged thirteen, whose brother had been a patient on account of hypermetropia with astigmatism. Having known that the sister had suffered severely from several attacks of chorea, I requested of her mother the privilege of examining the girl's eyes, which the mother readily granted. Accordingly the girl came to my office accompanied by two young companions, one of whom had suffered as severely as herself from chorea. Both the girls who had had chorea could read Snellen xxx. at twenty feet, but neither could read Snellen xx. at the same distance; while the third little girl who had not had chorea could read the xx. with perfect ease. Convex 42 glasses did not aid either of the two in reading at a distance, nor did any glasses appear to improve vision at a near point. Further examination, however, the eyes being under the influence of atropine, showed both to be hypermetropic. The first of these children has since that time worn +36 glasses, and the mother, who was about to remove her from school on account of her nervous condition, is now satisfied that she can continue her studies.

[1] The cases thus recorded do not include all that have come under my observation during this time. Since the reading of this paper two such cases have been brought to my attention, for whom I had prescribed convex glasses, but which had been forgotten.

Knowing that the daughter of a friend in Amsterdam, thirty miles from Albany, a child eleven years of age, had suffered several very severe attacks of chorea, I went to that town for the purpose of examining the child's eyes, and found that she had hypermetropia $\frac{1}{30}$ with sight equal to $\frac{16}{20}$. While at Amsterdam, I called upon my friend, Dr. W. H. Robb, of that town, who kindly offered me an opportunity of examining three other cases. Not having time to see them then, I took the first opportunity to return to Amsterdam to test the refraction of these persons. One of these patients, a girl ten years old, had chorea very severely five years ago. Her speech is still so imperfect that I could scarcely make out what she was saying, except as her mother interpreted her replies to my questions. She had vision $\frac{20}{30}$ at twenty feet, and hypermetropia $\frac{1}{30}$. The second case to which the doctor introduced me was a girl aged twelve, who had chorea three years ago, and has had repeated attacks since. She walks with a halt in her gait, her joints are large, she has slight strabismus, and hypermetropia $\frac{1}{20}$. The third case was a young lady aged twenty who had had four or five attacks of chorea, each lasting about two months. The left side was in each attack affected, and she cannot, even now, use the left hand well. She still has the restless uneasy movements of one who has never fully recovered from chorea. She had myopia $\frac{1}{14}$ with a high degree of astigmatism. Not having with me conveniences for making exact tests in this last respect, the precise degree was not ascertained. A day or two after seeing these cases a man was ushered into my consulting-room, who from his gait and manner I supposed was drunk. I was about to direct the attendant to remove him when a closer observation showed that he was the subject of some peculiar nervous trouble. He was allowed to remain, and said that he had come on account of ingrowing eyelashes. He was twenty-six years old, a laborer, feeble-minded, and evidently a subject of chronic chorea. He had the characteristic features of a hypermetrop, and I found his hypermetropia to be $\frac{1}{12}$.

Remembering that five years ago I had seen a girl at the

hospital who was suffering from a bad attack of chorea, I recently sought out her father's house, and called to see her. The mother of the girl, who received me, informed me that the daughter, who was now twenty years of age, was not in, but that she was still somewhat nervous. On stating that I would be glad to examine the daughter's eyes, I was somewhat surprised to hear the mother say that her daughter could not go on the street without glasses, and I was still more surprised to learn that I had myself prescribed the glasses five years ago—a circumstance which I had entirely forgotten. The young lady called upon me next day, and I found that she had myopia $\frac{1}{9}$ with astigmatism, requiring for its correction a cylindrical glass of forty-two inches focus.

Dr. Mereness, of Albany, kindly sent me the following case for examination:

Annie B., aged seven, had chorea a year ago, and was for many weeks helpless. The right side was apparently paralyzed. Her speech was so much affected as to render her quite incapable of expressing herself. The mother informs me that she is subject to much frontal headache, and that, after looking for a few minutes at her book, the tears start in her eyes. She has a high degree of hypermetropia, and the mother, who is thirty years of age, has only $\frac{20}{40}$ vision, and requires twelve-inch convex glasses for reading.

I am indebted to Dr. Beckett, of Albany, for the four following cases:

C. H., aged twelve, is suffering from her second attack of chorea. The present attack has lasted six months, and her condition is truly distressing. Her arms, legs, and body, are in perpetual motion, and her face has an idiotic expression. Her vision is reduced to $\frac{20}{30}$, and she has hypermetropia $\frac{1}{12}$. She has also astigmatism, but it is impossible to make out its degree. She has decided congestion of the optic disk and retina.

Maggie M., aged eleven, has had chorea four years. The first attack was the immediate result of fright. Her condition was then so bad that for many months, being unable to walk,

stand, or even creep, she rolled about the floor. She has never recovered fully, and is still suffering from violent twitchings of the limbs and body. She has hypermetropia $\frac{1}{15}$. Her sight, with and without glasses, is $\frac{20}{30}$, and, as in the preceding case, there is decided congestion of optic disk and retina.

H. B. is about twenty-five years old, feeble-minded, and subject to a most severe form of chronic chorea. He has myopia and a high degree of astigmatism.

Eliza L., aged 14, is now suffering her fourth attack of chorea. Her first attack occurred four years ago, and the succeeding ones have been about a year apart. The right side has been principally affected in all the attacks, the right leg and arm being nearly paralyzed in the first two. She has pain over the eyes, says that letters run together when she reads for a short time, and, on neutralizing the accommodation with atropia, hypermetropia $\frac{1}{30}$ was found to exist.

Dr. Smith, of Albany, permitted me to examine a patient, aged fourteen, who has had several severe attacks of chorea. He has hypermetropia with a slight astigmatism.

Dr. McKown, of Albany, was kind enough to introduce me to a relative of his, a lady who had, from the time she was fourteen until she was seventeen, suffered severely from chorea. She is now thirty-five years old. On my expressing the hope that our visit might not be considered an intrusion, she replied that she was glad to see me, as she had for a long while thought of consulting me in regard to the great pain she had for years experienced in her eyes. She was wearing +30 glasses, and had suffered from a severe form of asthenopia for years. I found her highly astigmatic.

My friend Prof. W. P. Seymour, of Troy, sent me for examination Alice T., aged thirteen, who has been subject to chronic chorea for several years. She has in the left eye myopia $\frac{1}{12}$, in the right eye myopia $\frac{1}{42}$; vision in left eye, when corrected by —42 glass, is $\frac{20}{50}$, in right, when corrected by —12, $\frac{20}{80}$. There is considerable congestion of the optic disk and retina of right eye.

Mrs. P., of Albany, aged sixty, has chronic chorea, and has hypermetropia $\frac{1}{11}$.

Mrs. Kate W., of Gloversville, came with her son to have his eyes examined, as he had chorea. It required but a moment to discover that the mother was suffering quite as severely as the child. On inquiry, I learned from her that she had acute chorea twelve years ago, and that the attack lasted severely for several months. A second attack occurred two years afterward, previous to the birth of the son who was now with her, and lasted during the whole term of gestation. She still has violent twitchings of the limbs, from which she has never been free since her second attack. She has hypermetropia $\frac{1}{40}$, and congestion of the optic disk and retina.

The boy Fred, ten years old, had chorea at birth, and has never been free from it. Has twitchings of the head, contortions of the face, jerkings of the limbs, and winks incessantly; is subject to headaches, and cannot be continued in school more than three weeks at a time, as he becomes so excessively nervous. His sight $= \frac{20}{30}$, and he has a hypermetropic astigmatism of $\frac{1}{42}$.

Frank A., of Lowville, New York, who consulted me three years ago on account of intraocular hemorrhage of the right eye, called upon me, a few days since, to have the other examined, as it was giving him some pain. Observing that he was in a perpetual motion while speaking, I questioned him respecting the commencement of his nervous trouble, and was told that in the autumn of 1868 he had what was regarded as typhoid fever, and that when he recovered from the fever he had chorea. To use his own expression, he "was on hinges during the whole of the following winter," and, to all appearances, he has not been off them since. I found no indication of disease in his left eye, but hypermetropia $\frac{1}{30}$ existed.

Making a summary of these two series of cases, we have, in the first, 13 cases; in the second, 20 cases; total, 33 cases. Of these there is simple hypermetropia in 24 cases; hypermetropic astigmatism in 4 cases; myopic astigmatism in 4 cases;

unequal degrees of myopia in the two eyes in 1 case; total, 33 cases.[1]

In one case apoplexy of the retina existed.

In one case intraocular hemorrhage had occurred, but not during the acute attack, and in three cases decided congestion of the optic disk and retina was found.

We come now to inquire whether, even allowing the coincidence of anomalies of refraction and chorea, it is reasonable to suppose that such erroneous refraction can be the exciting cause of so serious a derangement of the nervous and muscular functions.

I apprehend that it will not be at all difficult to convince medical men, familiar with the mischiefs wrought by erroneous refractions of the eyes, that the most serious nervous derangements may result from such causes. To the general practitioner, who sees less of these evils, this may be less apparent; but let such a practitioner who has himself normal eyes, and who is not himself yet presbyopic, attempt to read for half an hour with magnifying spectacles of sixteen or eighteen inches focal length, or let him study the landscape for a considerable time with a pair of glasses adapted to a near-sighted person, and he will soon, by his aching eyes, his nausea, and his vertigo, acknowledge that here is a cause of nervous disturbance of no slight or insignificant pretensions. And when we consider that convulsions and other serious and alarming nervous disturbances may be induced by the presence of worms in the intestines, or a hastily-devoured meal, or of a meal poorly digested, we cannot fail to perceive that this cause of irritation, prolonged through all the experience of the patient, is abundantly potent to such a result as chorea.

If we examine more closely the conditions of the eye in anomalous refraction, we shall be better prepared to understand the relations between them and disturbed nervous and muscular functions.

When the normal eye is at rest it has a clear and distinct vision of objects at a distance; that is, at twenty feet or more.

[1] For additional cases, see note at the conclusion of this paper.

As the object viewed approaches the eye, its refraction must be changed; otherwise the image would become dimmed and indistinct. The adjustment of the eye to different distances is performed, unconsciously to the individual, by the contraction of the ciliary muscle, through which the convexity of the crystalline lens is modified. The function by which the eye is thus adapted to different distances is called the function of accommodation. As the object viewed approaches more and more near, the ciliary muscle contracts more and more, until at a certain point it can contract no longer.

If a person with normal eyes brings the object very near them, and thus forces this muscle to unusual contraction, he finds that it soon fatigues, and he can no longer see the object well; for the maximum of the contractile power of this muscle soon exhausts itself.

In case of hypermetropia, clear and distinct vision, even of distant objects, can only be obtained by an effort of the accommodative faculty; hence, as the object approaches, the function of accommodation is more severely taxed than in normal eyes. It follows that, while normal eyes are always at rest when accommodated for distance, hypermetropic eyes are never at rest except when closed; and that, when viewing near objects, an excessive effort is required, and that this excessive effort is increased in proportion as the hypermetropia is more considerable.

Another important fact in this connection is, that, as the object viewed approaches, the eyes converge. This may be easily seen by any one who will watch the eyes of another before whom he moves a pencil or other object backward and forward, while the person observed fixes his eyes upon it.

The effort at accommodation and the effort at convergence are, therefore, simultaneous. They are also proportioned to each other; so that a pair of normal eyes accommodated for twelve inches are also converged for twelve inches; but, in the hypermetropic person the balance is lost, and such person who accommodates for twelve inches requires an exertion on the part of the ciliary muscle equal to accom-

modating the normal eye for a nearer point—we will say, for six inches. In such a case, a corresponding convergence of the eyes will take place, and so, while the eyes are accommodated for twelve inches, they are converged for six. Confusion of nervous and muscular action is the result, and to this confusion are due the pain, nausea, and vertigo of the average youth who attempts to read with his grandfather's spectacles, and this is the constant state of confusion of nerve and muscle in cases of anomalous refraction.

Let it also be borne in mind—and it is a point to which I am not aware that attention has been called—that young children have their eyes almost always accommodated for near objects. The baby has no interest in the landscape: he plays with his toes, he gazes admiringly at the raspy finger of his nurse Peggotty, held a foot from his nose, or he clinches his rattle in his tiny hands and stares at its glistening surfaces; at three, four, and five, he looks at pictures and toys; at six and seven he is sent to school, where he is forced to look at books, and it is only at a later period that he manifests much interest in distant objects.

Thus it is that in earliest years the ciliary muscle is in a constant state of extreme tension, and that in case of hypermetropia there is a perpetual conflict between this muscle of accommodation and the muscles of convergence, except, as it often happens, that there is a compromise by which one of the eyes agrees to a permanent squint, thus affording much relief to the little sufferer and great distress to its anxious parents.

It is while this irrepressible conflict is going on, and while the child with its impressible nature is suffering from this constant fatigue and irritation from muscles overtasked by extreme tension, and vexed by their failure to harmonize, that he is overworked at school, or is overtaken by measles or whooping-cough, or other infantile disease, and while thus debilitated is made an easy victim of the nervous irritation from which he has never been free. Or, perhaps, while on the verge of a loss of control over his muscles, a sudden fright

or disappointment becomes the last infliction beyond which the tired and exhausted nervous system can endure no more.

Another point remains for us to clear up: How are we to explain by such an hypothesis the phenomena observed by the distinguished Doctor Hughlings Jackson, and their absence as observed by others? This point seems to me to be most easily disposed of by our hypothesis. The functions of the ciliary muscle and those of the converging muscles are controlled by the third nerve, which has its deep origin in the region in which Dr. Jackson finds his lesions. To one acquainted with the phenomena of sympathetic ophthalmia the appearance of such lesions in such a locality will not, I apprehend, appear exceptional. Here an irritation resulting from a slight injury to the ciliary region of one eye, received perhaps years before, is transmitted backward, through the nerves supplying the ciliary muscle, to their deep origin and union with the nerves of the opposite side and forward along the corresponding nerves to the uninjured eye. The ciliary body of the uninjured eye becomes affected, the choroid and retina are subjected to degenerative changes and hemorrhages, and the sight is lost.

If, now, such changes may be induced in a previously healthy eye, by an irritation transmitted through two sets of nerves from the other eye, in which a minute foreign body may have been lodged, may not degenerations and lesions be also induced at the origin of these nerves as the result of continued irritation and vexation at their peripheral extremities.

Other arguments might easily be brought to prove the potency of such a peripheral irritation as that caused by anomalous refraction to induce the reflex phenomena of chorea, but I apprehend that enough has already been advanced to establish the proposition; and I have now to conclude this part of my subject with another proposition—a proposition which I have well considered and which I am prepared at the proper time to support with abundant facts and arguments, namely: That functional nervous diseases of all kinds are, beyond any

and all other causes combined, dependent upon anomalous refraction of the eyes.

Even if time allowed I need say but little respecting the treatment of chorea. The first and great indication is to correct the faulty refraction by the use of proper glasses. This will often, according to my own experience, relieve the patient at once. If glasses for any reason cannot be supplied or used, the child's eyes may be covered, for it is a well-known fact that when the patient with chorea sleeps the choreic movements often cease; this is doubtless because the eyes are at rest.

The Calabar bean has of late years been found of value in the treatment of chorea; and, when we remember that its peculiar and characteristic effect is to stimulate the ciliary muscle, we shall see that its use is rational.

As children and others suffering from chorea usually also suffer from general want of vigor, tonics, chalybeates, and arsenic are generally indicated.

I have thus, Gentlemen of the Academy of Medicine, endeavored to present my views of what seems to me to be a most important subject.

I rejoice at the opportunity of submitting these views to a body of physicians so distinguished as this, for I know that if there are errors in my reasonings or observations you will detect them, and that you will prove the correctness of this doctrine if it is founded in reason; and I dare to hope that this explanation of what has been a most obscure subject may not only be accepted, but that this line of investigation may throw light upon some other obscure nervous diseases.

NOTE.—*June 26th.* Since reading this paper before the ACADEMY OF MEDICINE, June 15th, I have examined eight other subjects of chorea with the following results:

Case I. W. V. O., Hypermetropia $\frac{1}{20}$.
" II. M. M., Hypermetropic astigmatism $\frac{1}{42}$.
" III. M. T., Hypermetropia $\frac{1}{30}$ + astigmatism $\frac{1}{60}$.
" IV. N. L., Myopic astigmatism $\frac{1}{42}$.
" V. K. H., Hypermetropia $\frac{1}{24}$.

Case VI. Miss T., Hypermetropia $\frac{1}{15}$.
" VII. Mrs. W., Myopia, right $\frac{1}{4\frac{1}{2}}$, left $\frac{1}{5}$.
" VIII. J. T., Myopia $\frac{1}{12}$ and $\frac{1}{14}$.

Adding these to the cases above reported and we have forty-one cases of chorea, in every one of which anomalous refraction occurs, as follows: hypermetropia 27; hypermetropic astigmatism 6; myopic astigmatism 5; unequal degrees of myopia 3; total, 41.

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